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LECTURES DELIVERED IN THE ROYAL INSTITUTION,
MAY 1905 TO JUNE 1907

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THE SLING

REMARKS IN CONNECTION WITH

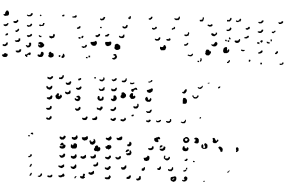
LECTURES DELIVERED IN THE ROYAL INSTITUTION

MAY 1905 TO JUNE 1907

BY

WM. LEIGHTON JORDAN

MEMBER OF THE ROYAL INSTITUTION; ASSOCIATE OF THE INSTITUTION OF CIVIL ENGINEERS; FELLOW OF THE ROYAL GEOGRAPHICAL SOCIETY, OF THE SOCIETY OF ARTS, OF THE ROYAL STATISTICAL SOCIETY, AND OF THE ROYAL METEOROLOGICAL SOCIETY; PAST PRESIDENT OF THE ENGLISH LITERARY SOCIETY OF BUENOS AYRES



LONDON

SIMPKIN, MARSHALL, HAMILTON, KENT & CO. LTD.

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PREFACE

In June last I printed for private circulation letters which I had addressed to the Royal Institution during the previous twelve months. After those letters had been printed I made the experiments which, in that of 23rd June, 1905, I had asked the Royal Institution to make, and the result is explained in my letters of 1st and 7th December, 1906, and in Appendix II. Some preceding letters which were made obsolete by those experiments are now omitted, and other letters added extending the review of the Institution Lectures to the following session.

The publication of the letters, not written for that purpose, seems to be justified by the importance of the errors attacked and of the new views suggested. The new discoveries in connection with the Sling, and their bearing on the subjects of 'Radiation Pressure' and the Pendulum would, even if they stood alone, suffice, I think, to justify the unusual form in which I have criticised the Royal Institution

Lectures; but the reader will, I hope, easily see that they do not stand alone as points of vast importance in the history of natural philosophy.

Nec Deus intersit nisi dignus vindice nodus
inciderit.

ROYAL SOCIETIES CLUB,
21*st October*, 1907.

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LETTERS

To Sir William Crookes, D.Sc., F.R.S., Secretary, the Royal Institution.

15th May, 1905.

Dear Sir,—Last Friday evening's lecture in this Institution constitutes an astounding occurrence. The able lecturer deserves our best thanks for his part in the matter, but, nevertheless, the fact remains that you have brought over from the United States one of our clever cousins to proclaim in our Lecture Hall, as a new discovery, a fact which had already been discovered here and established by explicit demonstration; whereas last night's treatment of the subject, instead of leading logically from cause to effect, was an avowedly unsuccessful attempt to apply a conjectural cause to phenomena rightly declared to exist, but which I am sure would have still remained unknown if an antecedent discovery of the true action of the forces on which they are dependent had not logically led to their discovery.

In astronomical questions, the force the lecturer dealt with as Pressure due to Radiation is, in fact, a combination of the transverse action of gravitation and reaction of astral gravitation which resists centripetal actions of gravitation; and without the antecedent discovery of that transverse action a knowledge of the effects could scarcely have been arrived at. The Professor's argument is brought forward only after the discovery of the fact that if any planet were lighter than it actually is, the sun's direct

force of gravitation would not be able to control its own transverse action in that planet's orbit, and the sun's transverse action would therefore lift the planet away from the orbit in which the heavier body retains its position.

The reason why the Professor was not able to show a clear logical connection between the facts he dealt with and Radiation Pressure is because no such radiation of heat through space of the character imagined does in fact exist. Interplanetary space is colder than ice. And it is only where the transverse action of the sun's gravitation acts on other bodies in space that heat is created by the clashing of their forces of gravitation. The sun's transverse action which creates the heat is itself the force which the lecturer treated as Pressure due to Radiation of heat. The heat is a consequence of the action of the forces by which the effects are caused, and not the cause of those effects. The idea of the sun parting with heat radiated across space to warm the earth is an exploded fancy. The sun, in fact, gets from the action of the earth's gravitation on it as much heat as the earth gets from the sun's action. Action and reaction are equal as regards that creation of heat in the two bodies.

The lecturer's argument to the effect that Newton's law of gravitation requires modification, shows him to be completely 'on the wrong track.' What that law has required is *extension* and not modification; and extension has been effected by the discovery of the action of astral gravitation and that of the transverse action of gravitation, with neither of which the lecturer professed to be acquainted.

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

To Sir William Crookes, D.Sc., F.R.S., Secretary, the Royal Institution.

24th May, 1905.

Dear Sir,—The subject dealt with in my letter of 15th instant is of such vast importance in the history of science that I wish more explicitly to define some of the points at issue.

I say that $\frac{mv}{r^2}$ (m = mass; v = radial velocity; and r = radius) is a universal formula for Newton's law of gravitation; and that the idea of the phenomena discussed by Professor Nichols, necessitating a modification of that law as he supposes, is erroneous.

I also say that $\sqrt[2]{\frac{mv^2}{r}}$ (v in this case being velocity of orbital motion) is a universal formula for orbital motion.

In the latter formula m is the central mass of any system together with that of the planet to be controlled by it. Taking m as the mass of any planet, mv^2 is the vis viva of orbital motion which has to be controlled by the centripetal force of gravitation. And as, in fact, the ratio which mv^2 bears to $\frac{mv}{r^2}$ increases as r increases, the sun's gravitation cannot control the vis viva of orbital motion without increments of planetary mass proportional to each increase of planetary distance from the sun.[1] The fact that the existing planets are controlled in their orbits by the reciprocal force of gravitation acting between them and the sun, necessitates therefore, for that purpose, in addition to the known masses of the planets and asteroids, the existence of material circulating round the sun equal in the aggregate to not much less than the mass of

[1] This subject is further treated in Appendix I.

Saturn. And that material is wanted chiefly between the orbits of Mars and Jupiter inclusive.

Anything beyond the orbit of Neptune small enough to be a planet would either be annexed by Neptune as a satellite, or else be flung away from the system by the transverse action of the sun's gravitation unless given an unstable cometary orbit.

That excess of transverse action is what Professor Nichols treats as Radiation Pressure.

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

To Sir William Crookes, D.Sc., F.R.S., Secretary, the Royal Institution.

29th May, 1905.

Dear Sir,—If you have read with any attention the letters which I addressed to you on 15th and 24th instant, you cannot fail to have recognised that the discovery announced by our United States visitor on 12th instant is a detail of a vaster and more clearly defined series of discoveries already made here.

The opposition of the Royal Society to my views has thwarted direct recognition of them here, but a fragmentary echo from them brought back across the water was welcomed at the Friday evening lecture on 12th instant as a new discovery recently made in the United States.

This is not the first time that such an echo has gained attention in our scientific societies which were deaf to the original announcement made directly to them; and my present object is to point out another similar instance.

In a recent astronomical work based on lectures delivered in this Institution, Professor Turner tells us that

about the year 1875 the method of dealing with astronomical questions was revolutionised by an American astronomer who discovered that mathematical work connected with the subject could be simplified by taking as a normal basis of the motion of any heavenly body a circular orbit round its primary, instead of a straight line tangent to that orbit, which latter had previously been taken as a normal basis for calculations.

The 'revolution,' described by Professor Turner, was in fact suggested by me in the year 1866, and it is only because the Royal Society, under the guidance of Sir George Stokes, rejected a Paper on the tides which I offered to them in the year 1868 that the new method was neglected in this country and first given practical effect in the United States. In the year 1866 I pointed out that the *inertia* of the earth actually does keep it on the line of its orbit, resisting any motion from, as well as towards, the sun. The *momentum* of the earth's revolution tends to carry it off along a tangent to its orbit, but that momentum is incessantly resisted by the earth's inertia with which it has erroneously been confounded.

This is an important question in the history of science. And, in fact, my demonstration to the effect that inertia keeps the earth on the line of its orbit and does not tend to carry it along a straight line at a tangent to the orbit, preceded the abandonment of the straight line by practical astronomers as a basis for their calculations. The new method, which has revolutionised astronomy, justifies the argument I published in 1873 to the effect that—'the science of astronomy must be simplified and more easily extended by the use of this new power of reasoning from cause to effect than by the former process of demonstrating the necessary connection of concomitant effects

proceeding from an unknown cause'—and history will not long continue to imagine the new method to have been arrived at except as a consequence of the correction of the erroneous theory of inertia by which astronomers had previously been guided.

The Royal Society have availed of and adopted the discovery made through discarding the so-called First Law of Motion without acknowledging their mistake in heaping disparagement on my initial action, though it seems absurd to suppose that the new method would ever have been discovered except in the manner I arrived at it through first discovering that the action of astral gravitation makes any free action of that so-called First Law impossible throughout the universe.

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

To Sir William Crookes, D.Sc., F.R.S., Secretary, the Royal Institution.

5th June, 1905.

Dear Sir,—In continuation of the argument of my letter of 29th ultimo, I record that at the Friday evening lecture on 26th ultimo, Professor Bruhl, of Heidelberg, explained experiments which he considered proof of the dissolution of material atoms; and just two years ago, at another of our Friday evening lectures (19th June, 1903), Professor Curie, of Paris, had not only argued to the same effect, but also appeared to suggest that the phenomena which he described in connection with radium indicated a new material substance being in process of creation. Whatever the precise idea Professor Curie intended to convey may have been, the general tenor of his argument clearly showed that his authority could not

be cited against the statement made by Professor Griffiths at our Friday evening lecture of 20th February, 1903 (just four months previously), to the effect that 'no physicist now relies with certainty on the conservation of matter.'

Professor Curie seemed to me to aim at a practical justification for that change in scientific opinion. At any rate, those lectures mark a clearly defined change of opinion since the time when Herbert Spencer fairly represented the scientific world in declaring that 'the indestructibility of matter is an indispensable axiom.'

Herbert Spencer goes on to say: 'The continuity of motion, like the indestructibility of matter, is clearly an axiom underlying the very possibility of a rational theory of evolution.'

Herbert Spencer on this point merely reflected what was, so far as I know, the unanimous opinion of the scientific world when, in the year 1866, I initiated the doctrine of the 'evanescence' in substitution for that of the 'indestructibility' of matter. Herbert Spencer's view was that of the scientific world not only in his day, but had been so from time immemorial. When Newton declared that in the beginning God made primitive particles so very hard as never to wear or break in pieces, he did not pretend to be expressing merely his own idea or that of a particular school; but he was in fact adopting, or leaving uncorrected, what was then an accepted axiom in science which had long before been tersely summed up by Jordano Bruno in the words: 'There is nothing which by natural fitness is eternal but the substance which is matter.'

The crusade I inaugurated against that doctrine in the year 1866 has triumphed throughout the scientific world. The evanescence of what were supposed to be

indivisible atoms, and not their eternity, is now a dominant basis of scientific argument, and has established a view of evolution vastly transcending anything dreamed of by the school of philosophy expounded by Herbert Spencer.

Notwithstanding that position of the question, the dictum of Sir George Gabriel Stokes and others to the effect that the views I had published were so wild as to place me utterly outside the pale of all reasonable scientific societies, seems to have so established itself in the minds of those even now in possession of our societies that the very men who are in fact proudly talking my dictation nevertheless fail to perceive that they are stultifying their position in the history of science by persisting in supposing me to be a 'paradoxer' properly outside the pale.

Dr. Williams, in his work 'The Story of Nineteenth Century Science,' alludes to Professors Stewart and Tait's inferences as to the destructibility of matter, and to Professor Dolbear's suggestions as to the creation of matter.

Dr. Williams there virtually gives to Stewart, Tait, and Dolbear credit for having originated the doctrine of evanescence which I announced, together with that of the retarding action of astral gravitation, in the year 1866.

My views as to matter being now in constant process of creation and destruction as a phase of force linking together pre-material and post-material forces, contributed quite as much as my denunciation of the idea of an 'initial push' given under conditions no longer existent, being a sufficient explanation of the existing planetary motions, towards setting in array against me all professional leaders of scientific opinion; and now, on those

views being found worthy of consideration, they are falsely credited to Stewart, to Tait, and to Dolbear.

I urge our managers to make some effort to change a course which must ultimately bring discredit on all who have held positions of influence whilst it has prevailed.

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

To Sir William Crookes, D.Sc., F.R.S., Secretary, the Royal Institution.

10th June, 1905.

Dear Sir,—Sir William White was almost the last man worthily enrolled among the leading *experts* of the scientific world from whom I expected such an ungenerous attack on 'amateurs' as he made in last evening's lecture; and, for several reasons, a protest on behalf of 'amateurs' may properly be addressed to you by me.

First, the man most eulogised by Sir William White was Fulton, who was essentially an 'amateur' when he framed the views which brought him into the position of the greatest expert of his age. When jeering at the mistakes made by 'amateurs,' Sir William White should have remembered that Fulton's achievements should (when distinguishing according to his method between experts and amateurs) be credited to the amateurs and not to the experts. I will not lengthen this argument either by adding other names or by specifying practical mistakes which may be debited to experts to make the balance even.

Secondly, one of the inventions illustrated last evening was ridiculed as showing the 'amateur's' ignorance of the true principles of wave motion. Does Sir William White pretend that any expert has yet mastered the

principles of wave motion created as a ship ploughs its way through the sea? Or, was the action of the tidal wave which accompanies every ship ever distinguished from the superimposed waves created by the ship's ploughing action before I (an 'amateur') addressed the Council of the Institution of Civil Engineers on the subject?

And, thirdly, in the year 1868 the Royal Society's experts rejected a Paper on the tides which I had based on a negation of the so-called First Law of Motion.

That law had never before that time been applied for practical guidance by shipbuilders. Only then Sir George Stokes and William Froude seized the idea that instead of discarding the law, as I proposed, it ought to be brought into more practical use.

I (an 'amateur') have given a clearly defined explanation of the manner in which erroneous theory, based on false principles, misled our shipbuilders into giving our yacht 'Shamrock III.' a shape which allowed her no chance against the American yacht 'Reliance' pitted against her by builders less enthralled by a theoretical fetish; and until your proud experts have given some better explanation than I have done, they would do well to refrain from throwing stones at 'amateurs.'

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

To Sir William Crookes, D.Sc., F.R.S., Secretary, the Royal Institution.

23rd June, 1905.

Dear Sir,—In my letter of the 10th instant I alluded to the manner in which I have challenged the principles recently adopted by our professional shipbuilders; and my

object to-day is to challenge, or, I should rather say, to express some doubt as to the correctness of generally accepted ideas regarding the action of a well-known instrument which, it appears to me, probably involves the same question as that at issue as regards resistance to a ship's way.

Though the SLING was a well-known instrument in war before the invention of gunpowder led to its displacement by the cannon for hurling projectiles, I find that text-books are generally shy as regards discussing the principles of projection it involves. Those which deal with it, however, expressly or tacitly, take it for granted that the vis viva with which the stone hurled from the sling strikes the object aimed at, is the kinetic energy created by the whirling motion and acting at a tangent to the whirl at the instant of the stone being released from the sling, supposing the motion to be circular at the instant of release.

I frankly state that I should like to have been able to make some more reliable practical experiments bearing on the subject than I have opportunity for, before suggesting that the above-mentioned idea may be erroneous; but, as that cannot be, I leave it to you and your brother experts to bring the question to a practical test when any one of you condescends to think it possible that mistaken ideas may perhaps prevail in text-books on the subject. I have corrected them on so many points that I do not grudge risking a mistake.

Regretting that I am not able to make such experiments as to enable me to be absolute one way or another, I venture to say that I incline to think that the kinetic energy with which the stone moved whilst in the sling supplies only half of the vis viva with which it strikes its object: the other half having been merely potential

energy whilst the stone was whirling in the sling, and becomes kinetic only on the instant of projection from the sling.

All the kinetic energy which existed in the sling at the instant of projection continues to act at right angles to that which was potential energy in the sling, and the tendency of the stone when released from the sling is to follow a diagonal between what were the tangent and the radius vector of its course at the instant of its release.

I suggest this diagonal, instead of tangental, course because, if found by experiment to be correct, the mistake which has existed regarding it explains why the fact of the sun's transverse action becoming uncontrollable by the direct action of gravitation in which it originates was not long ago recognised.

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

To Sir William Crookes, D.Sc., F.R.S., Secretary, the Royal Institution.

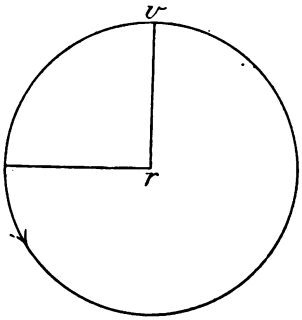
30th June, 1905.[1]

Dear Sir,—The main point on which I have addressed you is my contention that, in astronomical questions, $m v^2$ is the force which has to be controlled by $\frac{m v}{r^2}$ (v in the first formula being orbital velocity, and in the latter radial), and that the central mass, m, cannot control the actually existing transverse action, v^2, without planetary increments of mass increasing in the aggregate as the

[1] Some parts of this letter, and some other letters, recorded and commented on, misinterpreted experiments with the Sling, and are omitted in this edition. Appendix II. shows what the mistake was. The accepted theory of forces acting in the Sling required correction, and the mistake was a stepping-stone to that correction.

distance, so that not far beyond the orbit of Neptune any planet of sufficient mass or weight to prevent it from being hurled away from the system by the sun's transverse action would be destructive of the system.

Lord Rayleigh some time ago, in one of his lectures, showed us a whirling table contrived by Dr. Young, which made it quite clear that the centrifugal force requisite for effective action on the bodies arranged on it is $\frac{v^2}{r}$. The bodies are placed on it between rails, which allow freedom only along the radius vector. The leverage of the physical



radius, $r\,v$, not only supplies the motive force, but also prevents effective reaction against it, which has free play in interstellar space. That experimental mechanism of Dr. Young's, therefore, corroborates my argument to the effect that v^2 (and not $\frac{v^2}{r}$) represents the force which $\frac{v}{r^2}$ must control to prevent any body from being flung away from the central body of any system in space.

In Lord Rayleigh's experiment it appeared to me that the weight, when once started on its centrifugal course, traversed the radius vector without much effort to get away in either direction from it; and if so, then (as the

radius vector continued to revolve) the natural tendency of the weight must be to take a diagonal course unless it does not reach the circumference of the table until the radius has described an angle of 90°.

The arguments I have brought to bear on the subject suffice, I think, to justify me in urging you to have the questions at issue definitely cleared up by experiment in one of the laboratories of the Royal Institution without waiting for some clever cousin to cross the Atlantic to tell us of a 'new' discovery in connection with the sling, as was done in connection with my substitution of v^2 for $\frac{v^2}{r}$ as the centrifugal force surrounding the sun.

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

To Sir William Crookes, D.Sc., F.R.S., Secretary, the Royal Institution.

3rd July, 1905.

Dear Sir,—In my published works I have so explicitly shown that the reaction of Astral Gravitation against motion does *not* tend to alter Newton's tangental action of momentum, that a diagonal course of a stone from a sling, instead of the tangental, would contravene my own writings as much as those of any text-book; and I have this morning made experiments under a different arrangement to those on which I have already commented, with the result that, unless I kept the plummet in the circle through about 45° *beyond* the point at which I endeavoured to release it, the course taken is along the tangent and not the diagonal.

My object has been to find how Newton's treatment of the subject can be reconciled with the fact of a transverse action of the sun's gravitation increasing its ratio

to the direct force as the distance increases, so that it ultimately becomes uncontrollable by the latter.

I am glad for the present, in the absence of direct evidence to the contrary, to be able to adhere to the tangental course of the stone from the sling; and I give, not as a mere suggestion, but absolutely, as the explanation of the apparent anomaly which on writing my letter of 23rd June I was bent on clearing up, that, under Newton's treatment of the moon's motion (which ignores any reaction against the motion) a velocity of 55 feet per second should suffice to counterbalance the earth's gravitation in the moon's orbit; but the effect of the reaction is that the normal velocity has to be multiplied by the radius, making it 3,334 feet per second, in order to counterbalance the earth's direct action.

That same explanation holds good for the planets in relation to the sun, and shows how the velocity caused by the transverse action of the sun's gravitation gradually becomes too great to be controlled by the direct action in which the transverse action originates.

Our American cousin gave us in his lecture of 12th May, as a 'new' discovery made in the United States, a fragment of that excess of transverse action which had already been discovered here by a member of the Royal Institution.

I am, dear Sir,
Yours truly,
WM. LEIGHTON JORDAN.

To the Secretary, the Royal Institution

21st August, 1905.[1]

Dear Sir,—The 'second point' alluded to in my letter of 12th instant and left in abeyance, is as to the

[1] See footnote, page 12.

practicability of arranging an experiment to test the relative stresses along the radius A B and the tangent B T (Diagram, Appendix II., page 59), when a plummet at B is revolving in a circle B F.

It seems to me that there must always be a sufficient excess of tangental force to keep incessantly pushing the revolving body from the diagonal B F to the circle B F. The motion along the circle appears to be the resultant of the tangental exceeding the radial stress as 146 is to 100; giving relative velocities as 121 is to 100.

To have a concrete, instead of merely an abstract, illustration of the question at issue, let B represent the moon revolving round the earth at A. The moon traverses the quarter circle B F in 637,848 seconds, with a mean velocity of 3,334 feet per second; and consequently a mean velocity of fall of 2,123 feet per second from the tangent B T. But the force giving a tangental velocity along B T equivalent to the radial velocity B A would give a resultant motion along the diagonal B F. In order to lift the moon from the diagonal into the line of the circle the tangental force must be such as to give a velocity of 2,570 feet per second. The resultant motion of 3,334 feet per second is the square root of the sum of the two forces represented by $2{,}123^2$ and $2{,}570^2$.

The falling motion effected in the 637,848 seconds is not only the length of the radius, but also the incessant counteraction of the excess of the tangental over the radial force.

As those two forces are the radial and the transverse actions of the earth's gravitation, the resultant 3,334 feet per second is the effect of the full force of the earth's gravitation acting in the moon's orbit and giving that mean velocity of motion.

The foregoing is in practical agreement with Newton's

demonstration of the force of the earth's gravitation in the moon's orbit; for the radius 1,261,000,000 feet shortened to 1,245,000,000 feet to reach the common centre of gravity in which the moon and earth would meet in falling together, and divided by ·00447 of a foot shown by Newton to be the radial fall per second in the moon's orbit, makes the distance $2{,}785 \times 10^8$ times greater than the fall in the first second; and the square root of that time gives 527,800 seconds as the time for falling under the unimpeded action of the centripetal force acting between the earth and moon to their common centre of gravity; but the lifting action of the excess of tangental force increases that time as 121 is to 100, making it 527,800 × 1·21 seconds = 638,600 seconds,[1] approximating to the time actually taken for the average tangental fall in each lunar quarter sufficiently for the purpose of the present argument.

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

To Sir William Crookes, D.Sc., F.R.S., Secretary, the Royal Institution.

8th January, 1906.

Dear Sir,—First, I am disappointed by the non-appearance of any official report of the very interesting lecture delivered in this Institution on the 12th May of last year, which was the subject of my letters of 15th May and some subsequent dates; and my attention has been

[1] In a letter addressed to the Institution of Civil Engineers on 13th November, 1905 (given in Appendix III. on page 62), I gave a more direct and simple reason for this difference of period. See also letter of 3rd May on pp. 32, 33. Newton ignored the excess of tangental over centripetal force, and balanced that error by measuring orbital velocities along chords of arcs instead of along the arcs actually traversed. I have carried the investigation far beyond what appears in these letters.

drawn back to the subject by the beautifully arranged illustration of a comet's motion shown by Professor Turner on 28th ultimo, in one of the series of lectures with which he is at present delighting old and young in our lecture hall.

I have in my published works from time to time pointed out that the interaction of the sun's radial and transverse forces of gravitation tends to pull each planet to pieces and distribute its particles in a ring round the sun. Each planet resists that action by the dominance of its own cohesive force of gravitation.

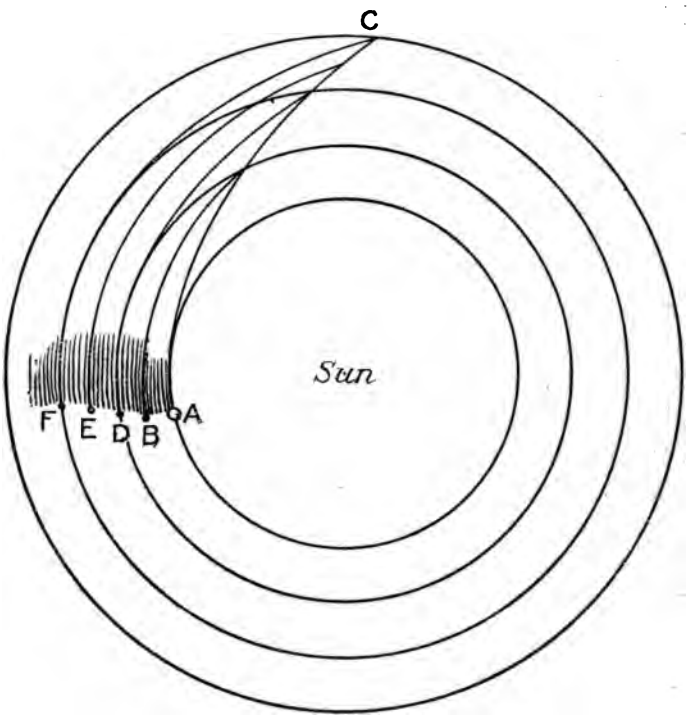
With the rapid increase of the ratio the transverse bears to the radial force which results from the motion of the comet plunging *across* instead of travelling comparatively near to the plane of the sun's equator, the formation of the comet's tail seems to accord with the idea of particles least closely adhering to the nucleus being torn away from it and prevented from so closely approaching the sun as the main body of the comet. The accompanying sketch shows the relative motions of the nucleus and the lighter particles torn from it.

The formation of the comet's tail seems fairly to confirm the transverse action of the sun's gravitation which I first deduced in the year 1870 from the orbital motions of the planets.

Secondly, an intrinsically amusing episode in Professor Turner's lecture on 30th December probably escaped your notice, and as it serves to show how unconscious those who are reputed leading representatives of the Royal Society are of some recent advances in our knowledge of physical phenomena, I think it permissible for me to call your attention to it; though I would not do so if it were not that it has also a serious aspect which serves to show how unfairly I am disparaged by those who are mistaken

as regards the simple matters of fact on which they base the influential detraction against which I have for many years struggled.

In the course of one of his experiments the lecturer accidentally touched a pendulum which had been fixed



C A Path of nucleus.
C B
C D
C E
C F
Paths of successive streams of dust which form the tail. (These streams are not in the same plane as C A.)

in readiness for a subsequent experiment; and, when the time for the pendulum experiment arrived he expressed regret for the accident, fearing that it might have prevented the intended demonstration from being given.

A wave of sadness passed through the juvenile listeners as the lecturer pointed out with regret that the pendulum did not swing quite straight; but that was quickly dispelled in a storm of delight when he announced that it was nevertheless moving in the right direction.

Well, in the first place, suppose the disturbance had been sufficient to make the pendulum begin by revolving in the wrong direction; the fact is it would, after a few swings, get rid of the bias accidentally given, and then automatically change to its right direction, and the desired demonstration would commence when the pendulum ceased to go the wrong way. Secondly, mathematically speaking, the curving from a straight line of vibration by which the juvenile audience were for a moment distressed is in fact an intrinsic part of the action of the forces by which the phenomenon of the revolution of the pendulum against the earth's motion is caused, though by steady starting with a long cord and short swing the deviation from the straight line is not immediately apparent; the action, however, steadily accumulates until the curve becomes apparent. Thirdly, for the purpose of the lecture it was unnecessary to keep the pendulum hanging in position for some hours previously and then to burn the string which held it in position. The pendulum may quite well be left hanging vertically, or swinging about, until wanted for the experiment; and if then drawn by the hand firmly against a starting-point marked on the heavy lecture table, as steady a start may be made as from the position of loose suspension from two cords which are always subject to more or less vibration.

The eminent representative of the Royal Society you enlisted to educate our juveniles is not up to date in the theory of the pendulum.

Thirdly, I now pass to a serious aspect of the performance.

In the year 1868 the Royal Society rejected a Paper in which I gave reasons why the main flow of the tide waves round the Great Southern Ocean should be eastwards—against the apparent motion of the sun and moon—and in which I claimed that existing tidal records confirm those theoretical views.

There can be no doubt that Professors Stokes and Huxley, then secretaries to the Royal Society, were honestly and firmly convinced not only that my views were, in theory, wildly erroneous, but also that, in fact, the tide wave does not flow eastwards as required by my theoretical views.

A wave following the moon westwards round the Great Southern Ocean formed an important basis in all their accepted tidal theory. I have, however, shown that the Admiralty tide tables thoroughly confirm the eastward motion, and therefore call for a change of attitude on the part of the Royal Society towards the theoretical views which first indicated the existence of such a motion when it was generally taken for granted that those tide waves flow westwards as depicted on Dr. Whewell's charts.

I have elsewhere pointed out that those who conducted the oceanic explorations for the British Government after the publication of my work on the Ocean did not understand the question at issue. They, in fact, found vertical differences of temperature according with the theory of the circulating action of astral gravitation, but utterly at variance with preconceived ideas both of theory and of fact; and in the first published reports those differences were smoothed away on the supposition that they were merely consequences of rough treatment of the thermometers.

The treatment of the pendulum by Professor Turner in the Juvenile Lectures is analogous to the treatment of the thermometer by Sir Wyville Thomson and Dr. Carpenter in the Government Explorations. Professor Turner treated as due merely to rough treatment of the pendulum phenomena which really constitute the most exquisite part of the effect of the earth's motion on it; just in the same manner as Sir Wyville Thomson and Dr. Carpenter treated as due to rough treatment of the thermometer phenomena which really constitute the most exquisite illustration of the conflicting action of terrestrial and astral gravitation in the ocean, which is caused by the earth's motion.

The attitude of Professor Stokes and other leading members of the scientific world towards my views is well shown by the following statement by Dr. Carpenter in the 'Contemporary Review' of September 1875.

Dr. Carpenter there says:

'Much ink has been wasted in the discussion of a question which the common sense of anyone who rightly apprehends the fundamental principles of physics should enable him to answer at once, viz., the influence of the earth's rotation upon the movement of the water which fills its ocean basins. This influence, supposing that water to be otherwise stationary, will be simply *nil*; for the water lying under each parallel will have the same rate of rotation from west to east as the solid earth in that parallel.'

The Secretary of the Royal Geographical Society thirty years ago informed me that the Council had decided that it was not desirable to encourage anyone engaged in commercial pursuits to meddle with scientific questions. That was also clearly the attitude of the Royal and other societies, whilst at the same time the

above extract from the 'Contemporary Review' shows how I was handicapped in those commercial pursuits by the mistaken action of leading members of the scientific world.

The treatment of the pendulum in the Juvenile Lectures above alluded to, combined with that of comets' tails in the lecture of 12th May last year, entitles me, I think, to ask the managers of the Institution to make some effort to change the attitude of the scientific world as regards the action of astral gravitation in terrestrial phenomena.

I am, dear Sir,
Yours truly,
WM. LEIGHTON JORDAN.

To Sir William Crookes, D.Sc., F.R.S., Secretary, the Royal Institution.

3rd March, 1906.

Dear Sir,—In his clearly expressed and extremely interesting treatment of the upward flow of sap in plants, Mr. Francis Darwin on Thursday afternoon declared the action to be still unexplained, as the fluid is, in fact, lifted in tall trees to a greater height than is compatible with the pumping action caused by the creation of a vacuum, and all efforts to trace a succession of pumping stages have failed.

I would not address you for the purpose of asking our managers to weigh any opinion of mine against that of Mr. Darwin on any point on which he has a definite opinion, but I cannot allow the statement that the rise of the sap, even in the tallest trees, stands unexplained to pass unchallenged in the lecture hall of the Royal Institution.

I say that the attempted explanations dealt with by Mr. Darwin have failed because the idea on which they are based, to the effect that the action is analogous to that of the vacuum created in a common pump, is erroneous. If there is any 'vacuum action' in the circulation of the sap, it must exist in infinitesimal detail, and only as a consequence of the direct action which is the true motive force.

Water is raised within a mountain by molecular gravitation preponderating over gravity, and is thus enabled to issue from the mountain side high above the reservoir from which it is supplied. Sap not only rises vertically or flows horizontally by the same molecular action of gravitation, but also has, by some as yet unexplained mechanical action, the power of building channels by which it lifts itself, by the action of its molecular gravitation, away from the ground. And blood has the still more highly developed power of completing a circulation in a single body by building channels for a return circulation which enable it to attain to some freedom of action by breaking away from visible bonds connecting it with the earth.

And now, sir, I must join issue with Mr. Darwin as regards a point on which he failed to do justice to a simple and thoroughly scientific opinion expressed by St. Paul. St. Paul argued that the perishing of the body is not proof against the continued existence of the soul, and gave as an analogous illustration the perishing of a seed resulting in the growth of a tree. A fair interpretation of his argument seems clearly to be a contention on his part to the effect that the perishing of the seed does *not* destroy its life, as is proved by the growth of the tree. It is only from that point of view that he is able to assert analogy as to the continued existence of life

after the death of the body as well as after the death of the seed. Mr. Darwin's criticism of St. Paul seemed really to depend on a technical interpretation of the word *death* quite at variance with the idea which pervades St. Paul's argument. His argument is to the effect that the death of the *seed* is not the death of its *life*; and his view as to the breaking away of life from the body is merely a natural step following after the breaking away of the living body from the earth.

I beg, sir, that you will not lose sight of the fact that it is Mr. Darwin, and not I, who has brought St. Paul before the Royal Institution; and I take up the challenge because for forty years my scientific views have been frowned on by reputed leaders of Science, under the mistaken idea of agreement with St. Paul on the above point being unscientific.[1]

I am, dear Sir,
Yours truly,
WM. LEIGHTON JORDAN.

To Sir William Crookes, D.Sc., &c., Secretary, the Royal Institution.

11th April, 1906.

Dear Sir,—It has become clearly inexpedient for me to refrain from addressing you on the subject of Professor Thomson's Lectures of Friday, the 19th January, and the Saturdays of 3rd ultimo to 7th instant, as illustrating and confirming the general tenor of the letter which I addressed to you on 5th June of last year.

I then quoted Herbert Spencer's statement that:—
'The continuity of motion, like the indestructibility of

[1] A letter closely connected with the question of the upward flow of sap in plants is given in Appendix IV.

matter, is clearly an axiom underlying the very possibility of a rational theory of evolution.'

That statement fairly represents the basis of scientific opinion as it existed when, in the year 1866, I began my public crusade against both of Herbert Spencer's so-called axioms.

What now has become of the doctrine—the 'axiom'—of the 'indestructibility of matter'?

The tenor of Professor Thomson's lectures made it appear that the doctrine of the *evanescence* of matter, rather than its *indestructibility*, is one of the foundation stones of his philosophy.

At any rate, even if Professor Thomson has not consciously advanced so far as to adopt the doctrine of evanescence unmarred by trivial limitations, he certainly has abandoned the so-called 'everlasting atoms' to destructive forces.

He did not appear to base any argument on the question as to whether those 'elementary atoms' are evanescent or indestructible, but, taking it for granted that they are evanescent, he attempted to explain possible processes under which they dissolve and the consequences of their decomposition.

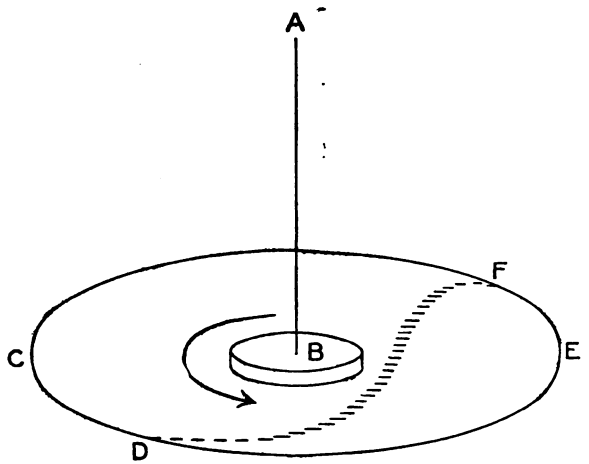
That is all very well; but together with it, the resuscitation of Newton's corpuscular theory of light is a minor episode in the part which the corpuscles supposed to combine to form Newton's 'indivisible' atoms are called on by their modern supporters to play.

Those minute particles are now called on, not only to dash through space from the sun to the earth at the speed of nearly 200,000 miles a second, but also to be, at the same time, flying in other directions for other purposes in the same field of action, and in such a manner as to

affect a heavy gyroscope so as to make it revolve faster in one direction than in the other.

I deny that there is any mystery as regards the force which causes the difference in the velocities of revolution pointed out by Professor Thomson, which, however, constitutes an interesting fact which has never before, so far as I know, been noticed.

Professor Thomson showed us that a suspended gyroscope, A B (in the Diagram), if tilted to the circle C D E F



and given a motion of revolution in that circle, will revolve faster in one direction than in the other.

I some years ago made some rough experiments for the purpose of testing the theory of astral gravitation resisting all motion in the ratio of the square of its velocity. According to that theory the resistance is greatest when the revolution is in the same direction as the rotation, because the two motions combine to give an extreme excess of velocity on the outside edge of revolution over that at other points of the disc. And I found that in fact

the disc, after revolving for a short time in the direction C D E F, became uneasy in its motion, and then crossed the circle, as along the dotted line F D, and continued its revolving motion, but in the opposite direction to the motion of rotation and more steadily.

The theory I then tested makes it appear to me that the faster of the two motions of revolution shown by Professor Thomson must have been that which was against the motion of rotation, though I did not notice that increased velocity at the time of my experiments.

Different conditions under which experiments are made must make the resulting effects differ, but until the simple action of gravitation resisting the motion of the gyroscope as the square of the velocity is proved to be unable to explain the phenomena under the conditions of the beautiful experiment placed before us by Professor Thomson, it is surely a mistake to appeal to mysterious action of mysterious 'corpuscles.'

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

To Sir William Crookes, D.Sc., F.R.S., Secretary, the Royal Institution.

27th April, 1906.

Dear Sir,—In my letter of 8th January, I commented on views expressed by Professor Turner in the course of his 'Juvenile Lectures.' I sincerely hope that my action in so doing is clearly recognised to be, *not* an attack on personal opinions of Professor Turner's, but on the whole school of science which he very ably and worthily represented. The high position which you, sir, hold in the scientific world makes my criticisms quite as applicable

to you personally as to Professor Turner; and still more strongly are those criticisms (made in a letter written in this Institution and addressed to you as Secretary) applicable to our official representatives of science, Lord Rayleigh and Sir James Dewar.

As, however, in the opening lectures of this year, Professor Turner was the protagonist of the old school of science in this Institution, I beg that, as a natural sequence to my comments on his lectures, our managers will not consider it altogether out of place for me to supplement my letter of 8th January with some comments on an article by Professor Turner which appeared in *The Times* on 24th instant, on 'The Times and Places of Earthquakes.'

1. Professor Turner tells us that the discovery of the existence of irregularities in the earth's rotation was made only twenty years ago.

As regards that statement, it is mere matter of fact that in April, 1879 (that is twenty-seven years ago), I addressed to Mr. William Spottiswoode, then President of the Royal Society, a Paper on the Moon's Motion, in which I declared such irregularities to be then 'a demonstrated matter of fact.' (That Paper forms now Chapter XIII. of 'The New Principles of Natural Philosophy': Longmans, 1885. See p. 250, line 17.) Let it be granted that Professor Turner is right to the extent that the great masters of the old school, who held complete control of the Royal Society, became aware of those irregularities only twenty years ago; then the fact is that the Royal Society tardily recognised the existence of a fact which the new principles had explained and announced to them seven years previously.

The theoretical action of forces which explained the facts before they were generally recognised is more

entitled to consideration than explanations invented to suit them after the evidence of their existence had become indisputable.

2. Professor Turner tells us that there are two contributors to our earth's rotation, which sometimes act in unison and sometimes in opposition. I do not know which are the two contributors to which Professor Turner alludes; but I do know that the two *principal* contributors (besides the earth's own normal force of rotation) are the transverse action of the sun's gravitation which carries the earth round the sun, and the transverse action of a force of gravitation which carries the sun round the centre of our stellar system.

As regards the transverse action of the sun's gravitation which changes its direction in relation to the earth's rotation in semi-annual cycles, and the velocity of motion in annual cycles, the outer crust of the earth has doubtless settled into a condition of stability. But that stability is disturbed by the interaction of the transverse force of gravitation which revolves our stellar system in a period of 26,000 years, giving a maximum and a minimum velocity in that period just as the sun's transverse action gives a maximum and a minimum velocity each year. The gradual change of the velocity of stellar revolution in the cycle of 26,000 years is apparently the cause of the present disturbances.

Besides the change of velocity, there is also a change in the direction of that revolution in relation to the earth's axial rotation and orbital revolution, which occurs twice in each of those cycles.

3. Professor Turner tells us Professor Jeans has suggested that 'the earth is by no means a sphere or a spheroid, as we have been accustomed to think, but is of a pear-shape'; and that 'the pear is being crushed

into a sphere by its own attraction; and the result is a series of earthquakes.'

It is now forty years since I published the work in which I pointed out that the motion of the earth through space *tends to give it a pear-shape*, and that the actual deviation of its shape from a true spheroid indicates that such motion is southwards—the Antarctic Continent rising from the Great Southern Ocean being the 'stalk' end, and the Arctic Ocean surrounded by the Northern Continents being the 'bluff' end of the pear. That should be the *longest* diameter in order to constitute a true 'pear-shape'; but, in fact, the oblate spheroid caused by the earth's rotation is not sufficiently disfigured to keep that line from being the *shortest* diameter of the 'pear'; and I applied the term cardioid to the shape which actually results from the interaction of forces.

Since I first pointed out that cardioid shape, Nansen has responded to my call for water in the Arctic regions by discovering a vast extent of deep sea there; and my call for land in the Antarctic regions has been responded to by Scott's discovery of King Edward VII. Land and a great extent of mountainous land in still more southern latitudes; whilst Drygalski has responded by discovering Gauss mountain, where he expected to find an oceanic pathway from the South Indian Ocean to the South Pole.

Professors Jeans and Sollas place the ends of their 'pear' in the South Pacific Ocean and in North Africa; but Professor Turner's question as to which is the 'stalk' end makes it appear that the pear must have become sadly squashed out of 'pear-shape.' It seems clear to me that they are in fact dealing with the ruins of a former cardioid, which existed when the direction of the earth's motion through space differed from what it now is in relation to the earth's surface.

If we suppose the last change of direction to have occurred at the time of the Noachian deluge, some thousands of years have still to elapse before another change of direction occurs; and even that mighty change of conditions in many parts of the world perhaps left China and some other regions comparatively little affected, in which case interesting records probably lie forgotten in the Mukden Library.

Be the merits of the latter point, however, what they may, Professor Turner's very interesting article brings into strong relief important points in scientific research where he and other masters of the old school have only recently begun to tread in footsteps which had long before been made by the new theory of Vis-Inertiæ.

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

To Sir William Crookes, D.Sc., F.R.S., Secretary, the Royal Institution.

3rd May, 1906.

Dear Sir,—I was greatly interested in the remarks you made on Tuesday at our annual general meeting on your position as honorary secretary. It appeared to me that they were perhaps inspired by the letters I have recently addressed to you; be that, however, as it may, they virtually formed a renunciation of responsibility on your part for the treatment of those letters, and, I think, an expression of opinion to the effect that not only the managers but also Sir James Dewar (as practically their representative) are responsible for conducting the affairs of the Institution. I am glad to think your remarks a step towards obtaining attention for the scientific questions I have brought before you; but, at the same time

I cannot see that you are entitled to repudiate responsibility as regards their treatment by the Institution. The Year-Book of the Institution, lying here on the table, declares you to be the 'Secretary.' My appeal is, therefore, naturally made through you as secretary; and, independently of that office, your position in the scientific world is such as to enable you to command attention, so that you cannot free yourself from personal responsibility.

With what I have placed before the Institution in works now in the library, and in these letters, we have probably elements sufficient, if dealt with by men of the mathematical knowledge and ability of Lord Kelvin or Lord Rayleigh, and perhaps some others among our managers and professors, to trace the orbit of our stellar system.

Two among many other important questions which will almost certainly be determined by the solution of that orbit (as they appear to be naturally dependent on it) are:—

1. The reason for what is otherwise an anomaly in the period in which the moon describes its orbit of revolution round the earth.

According to the data made use of by Newton in solving the law of gravitation, the moon falls ·00447 of a foot per second from the tangent to its orbit, and therefore falls in 6·1 days the length of the radius of the orbit it describes round the common centre of gravity of the earth and moon; thus describing a quadrant in that time, and a complete circle in 24·4 days, whereas the period of its sidereal revolution is 27·32 days, and that of its synodical revolution 29·53.

I know of nothing that can give a clear and simple reason for the first of the above three periods, unless the moon in fact returns each 24·4 days to similar positions in relation to the centre of gravity round which our

stellar system revolves. It then takes about three days to catch up with the motion of the visible stars round that centre of gravity, and about 2¼ days more to catch up with the motion of the earth round the sun.

2. The eccentricity of that stellar orbit serves to explain the existence of the glacial periods which have been indicated by geological researches. Existing evidence is to the effect that the solar system is now traversing the part of its orbit which corresponds to the vernal equinox of the Southern Hemisphere in the earth's orbit round the sun.

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

To Sir William Crookes, D.Sc., F.R.S., Secretary, the Royal Institution.

14th May, 1906.

Dear Sir,—I know it is dangerous to comment on an elaborate scientific argument before it has been placed on paper; but the main points of the important lecture delivered on Friday evening by Professor Poynting can scarcely be open, I think, to any misunderstanding.

In the first place, the argument clearly aimed at rebutting existing attempts to resuscitate Newton's corpuscular theory of light; and I venture to think it will prevent us from hearing any more talk of material particles dashing through space at the rate of nearly 200,000 miles per second.

If those tiny corpuscles cannot do the comparatively simple work which Newton demanded of them, still less can they reasonably be supposed competent to affect a heavy gyroscope in the manner required by their modern champion. Professor Poynting has indeed thoroughly

justified the comments I submitted to you on the very interesting and able exposition which Professor Thomson gave of the corpuscular theory in his recent lectures.

Secondly, Professor Poynting explained to us how he and others had succeeded in measuring the amount of force exerted by the pressure of light. I know no reason why any student of science should be otherwise than predisposed to accept and welcome such an announcement. But, when Professor Poynting goes on to say that the demonstration of a force of pressure exerted by sunlight constitutes a modification of Newton's law of gravitation, I join issue and renew the protest made in my letter of 15th May, 1905, when Professor Nichols made a similar claim. Newton's law remains perfectly true, no matter what other forces may be acting on any body at the same time.

When a book is lying on a table Newton's law is perfectly true as regards the book, even though the strength of the table prevents it from causing the book to move.

If Professor Poynting (also Professor Nichols) agreed with me that a transverse action of the sun's gravitation is the cause of its light, it would then be more plausible for him to urge that its light modifies Newton's law for the action of its gravitation. But the fact is that Newton's law must be treated independently of the interaction of any other laws, not excepting even that of the transverse action of the same force of gravitation. What requires modification is our estimate of the relative masses of the sun and the earth and that of any pebble lying on the earth, as pointed out in a letter which I addressed to the Royal Society on the 21st August, 1901. To modify Newton's law instead of those estimates would not only lead to endless complications, but is also an intrinsically erroneous

method of dealing with the question. Gravitation is as the mass and inversely as the square of the distance: and any transverse action of a part of the force does not modify that law, but forms a supplementary law.

Thirdly, as regards the book lying on the table: it is quite allowable for any one who chooses to do so to deal mathematically with the pressure existing between the book and the table instead of with the action of gravitation, which I (and I think most people) say is the cause of that pressure. But the law of gravitation, nevertheless, remains quite true as regards that book.

The transverse action of the sun's gravitation acting on the earth is tangental to the radius vector of the orbit, and is just as truly a *pull* along that tangent as is the direct action a pull along the radius vector; and pressure between the sun and the earth, whether manifested as heat, as light, or as electrical action, is a consequence of the reciprocal action of their gravitation just as truly as the pressure between the book and the table is a consequence of the reciprocal action of gravitation between the book and the earth.

So-called ether pressure in space is, in fact, merely the opposite aspect of (or a consequence of) the tension of astral gravitation which pervades the universe.

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

To Sir William Crookes, D.Sc., F.R.S., Secretary, the Royal Institution.

23rd May, 1906.

Dear Sir,—As the subject of the lectures to be delivered by Professor Sollas on the afternoons of the next three Thursdays are closely connected with that of

a very interesting paper contributed by him to the Geological Society, and published in the 59th volume of their Quarterly Journal (May 1903), and as I pioneered, forty years ago, some of the most important points dealt with in that paper, I submit some comments in sequence with the course of letters originated by the misappropriation of my work in the lecture hall of this Institution, as explained in my letters of 15th May, 1905, and some subsequent dates.

In the discussion of the above-mentioned Paper by the Geological Society, the President (Professor Lapworth) asked why those who inferred an original pear-shaped figure for the earth did not notice that the present spheroid of rotation is somewhat pear-shaped—the Arctic regions being the blunt end, and the Antarctic the stalk end of the pear.

The fact, sir, is that the present treatment of the subject in the Royal Society and in the Geological Society is based on suggestions which were scouted when I first published them forty years ago.

The tendency of the oblate spheroid to deviate towards the so-called pear-shape is shown in a diagram impressed on the cover of a book which I published in the year 1866; again on another book published in 1867; on a third published in 1868; on a fourth published in 1873; and it is repeated in the frontispiece of the Essays on the Action of Astral Gravitation which I published in the year 1900, besides being the subject of various diagrams in previous works. The importance I attached to the point is shown by the prominence I gave to it as above stated, and my opinion of its importance has become justified by the so-called pear-shape having become the subject of numerous Papers in the scientific societies of this and other countries.

I did not use the term 'pear-shape' because, in its original position, it would be a 'pear' broader than its length; and because it is not merely 'blunt' at one end, but has a subsidence at that end; and I therefore used the term cardioid. The conformation of the cardioid becomes elongated so as to be more in accordance with pear-shape by a change of its position in relation to the earth's rotation.

Professor Sollas points out that a line from South America to the East Indies gives 'roughly a pear-shaped form.' And, in fact, in 1866 I gave reasons for supposing that Brazil had formerly been an Antarctic Continent. In such case the then existing Arctic Ocean would have been more or less south-east of the Japan Islands. The nations of some parts of Asia would have had the same climate as under the now existing conditions; but the Japanese would have been the 'Eskimos' of that era.

I also gave reason for supposing that traces exist of changes antecedent to the above to account for the absence of more land in the South Pacific Ocean.

Professor Sollas supposes a pear-shape to exist with the stalk end in Northern Africa and the blunt end in the South Pacific, and that the recent succession of earthquakes is due to the action of gravity tending to crush that pear-shape towards a spherical form. I have a perfectly free and open mind for anything Professor Sollas may urge in support of that idea. The evidence on which my work has been based, however, indicates that at least about 19,000 years must have elapsed since a tendency to create a pear-shape in that position existed; and the now existing stresses between the forces which create the existing oblate spheroid and those which give it the existing deviation towards the cardioid, or 'pear-

shape,' alluded to by Professor Lapworth, give a readier explanation of the slight instability of the earth's crust now becoming a very general subject of consideration.

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

To Sir William Crookes, D.Sc., F.R.S., Secretary, the Royal Institution.

26th May, 1906.

Dear Sir,—*Verba volant:* warns me of the dangers which beset the path I am about to traverse in commenting on the important and intensely interesting lectures Professor Sir James Dewar has delivered on the last two Saturday afternoons on 'The Old and the New Chemistry.'

In commenting on Professor Poynting's recent lecture, I suggested that it would put a stop to the attempt to resuscitate Newton's corpuscular theory of light, considering his argument a death-blow to that attempt. Sir James Dewar, in refraining from standing by as a silent spectator of the tragedy, has (as it seems to me) shown a very proper sense of the responsibilities of the high position he has attained in the scientific world.

There did not appear to me to be too much ceremony in Professor Poynting's cuts and thrusts; but, even if he was rather heartless in the affair, Professor Dewar, acting as chief mourner, has amply atoned for it by the tones of regret with which in solemn and slowly-measured language he limited material particles to a velocity which would not enable them to move at more than two-thirds of the pace requisite to enable them to be the Newtonian corpuscles of light. The corpuscles are now, indeed,

doubly dead as regards their pretension to constitute light; but what has Professor Dewar to say of their pretension to be the controllers of the gyroscopic phenomena in the beautiful experiment which Professor Thomson placed before us in his recent lecture, alluded to in my letter of 11th April? That appears to me to be a more startling pretension than their claim to be light. Just think, sir, of those tiny, invisible particles being in a chronic state of dashing about the lecture hall with such fierce velocity as to create the phenomena of the gyroscope shown to us by Professor Thomson! I beg you, Sir William Crookes, together with your managers and their professors, to open your eyes and recognise that there is no basis nearer than the stars for the tension which controls the movements of the gyroscope.

There is much of interest in Sir James Dewar's lectures with which it would be imprudent to grapple before seeing it on paper. In general terms I may, however, I think, say that he virtually agreed with the views of Professors Bruhl, Griffiths, and Curie, expressed in our lecture hall and alluded to in my letter of 5th June last; and, passing to a more clearly-defined position than held by them, his exposition of the electrical basis of matter, combined with his limitation of corpuscular velocity of motion, is virtually a declaration of transmutations between material substance and immaterial force dealt with in my essay on 'Sir Isaac Newton and Modern Chemistry,' and constitutes an adoption of the doctrine of the evanescence of matter which I stood alone in upholding forty years ago.

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

To Sir William Crookes, D.Sc., F.R.S., Secretary, the Royal Institution.

14th September, 1906.

Dear Sir,—*Verba volant*: which I quoted in the last letter which I addressed to you, conveys a warning which could not easily be more splendidly justified than it has been by the abstract of Professor Nichols' Paper on Pressure due to Radiation, which you have just distributed among the members of our Institution and published.

The attack made on Newton's Law of Gravitation in our Lecture Hall on 12th May, 1905, remains: *vox et præterea nihil.* The able young Professor you put forward to lead the rebellion against that Law, on that which was evidently intended to be an epoch-making evening, has (if he is responsible for the abstract) thrown up the sponge and left the rebellious crowd who supported him on our front benches without an apparent leader. Will they all be content to subside like the foam of a wave falling back repulsed by an adamantine rock? Or, will you, sir, organise another attack?

Newton demonstrated the Law of Gravitation in the only manner in which it was possible for it to be arrived at: namely—by its direct action, and not through the varied forms of pressure which that action creates.

I again urge you, Sir William Crookes, together with your managers and their Professors, to open your eyes and recognise that Radiation Pressure in all its forms is merely the opposite aspect of the action of astral gravitation which pervades the universe.

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

To Sir William Crookes, D.Sc., F.R.S., Secretary, the Royal Institution.

21st September, 1906.

Dear Sir,—In a letter dated 14th instant I commented on the omission from the report in our Proceedings of what, when the lecture was delivered by Professor Nichols on 14th May, 1905, appeared to be intended by the lecturer to be the climax of his argument; which was certainly then declared to require a 'modification' of Newton's Law of Gravitation.

The report of the lecture leaves Newton's law untouched; and I now venture to make some comments on the *débris* which has been printed in our Proceedings.

The abstract tells us that the first speculations upon a possible pressure due to radiation were suggested by the behaviour of comets' tails; but it refrains from printing the verbal description of—'the enormous velocity with which the matter of the tails is carried backwards, in opposition to the motion which it had as part of the nucleus.'

In the absence of the words used by Professor Nichols I give the above from Sir John Herschel; and I notice that, applied to the comet of 1843, the argument would require a velocity of nearly 200,000,000 miles in nearly no time.

Sir John Herschel expresses a doubt as to whether 'it is really *matter*, in the ordinary acceptation of the term, which is projected from the heads of comets with such extravagant velocity'—'in defiance of the law of gravitation, nay, even of the received laws of motion.'

The fact is that (subject, perhaps, to a trivial correction) the so-called enormous and extravagant velocity is no velocity at all: the velocities are round the sun, *not*

from the sun. As the comet plunges across the plane of the sun's rotation the transverse action of the sun's gravitation takes complete control of the dust torn away from the comet; which latter continues its course under the *combined action of the two* forces (its own and the sun's); and as the comet returns to recross the sun's equator it gathers up its tail as the particles continuously meet the comet as the latter recedes again from the sun.

The action does *not* defy, but constitutes a verification of the laws of gravitation; and as for the so-called First Law of Motion, the comets laugh it to scorn. Herschel is right as to the comets setting it at defiance.

In the year 1866 (forty years ago) I gave that law an initial impulse which has set it going, with a velocity which is rapidly accelerating, to the limbo of exploded fancies; and if you, Sir William Crookes, and our managers, persist in holding on to it, it will take all your reputations with it.

I am, dear Sir,
Yours truly,
WM. LEIGHTON JORDAN.

To Sir William Crookes, D.Sc., F.R.S., Secretary, the Royal Institution.

24th September, 1906.

Dear Sir,—The abstract of Professor Nichols' lecture declares him to have given a satisfactory explanation of 'the hitherto mysterious behaviour of comets' tails.'

That alone, quite independently of the question as to whether that explanation did, or did not, necessitate a 'modification of Newton's Law of Gravitation,' was enough, if the explanation were sound, to make the lecture an epoch-making event in our Institution.

The boasted explanation has not, however, dared to show itself in print: *Scriptum manet*; and you, sir, must, I think, now be conscious of the sorry figure it would have cut if you had allowed me to get hold of it in that form.

Kepler did not allow himself to be hampered by the so-called First Law of Motion; and he seems to me to have been nearer to a true explanation of the phenomena than any 'orthodox' scientist has since been.

In face of the above I think it not out of place to remind you that on 13th of March, 1898, I appealed to the managers of our Institution not to allow the Nineteenth Century to close (with the 31st December of the following year) [1] without Astral Gravitation having been fairly discussed in any of the professional seats endowed for the advancement of scientific knowledge. In making that appeal I believe I was considering the general interests of science as much as the immediate question of fair-play for the extended laws of gravitation in their struggle against the so-called First Law of Motion.

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

To Sir William Crookes, D.Sc., F.R.S., Secretary, the Royal Institution.

1st December, 1906.

Dear Sir,—In my letter of 30th June, 1905, I appealed to you for enlightenment regarding two points in connection with the Sling; and I desire now to advise you

[1] In a small volume on 'Astronomical and Historical Chronology' I have shown it to be a simple historical fact that, when the Christian era was invented, what we now call 1 B.C. was the first year of the era and 1 A.D. the second year of the era; so that the first year of the era *closed* as 1 A.D. in our chronology *commenced*.

that I have just arranged mechanical experiments which appear to set both questions at rest.

First: the plummet released from the sling *does* follow a tangent to the curve in which it has been revolved; thus leaving generally accepted ideas quite correct on that point.

Secondly: the ratio which tangental bears to radial force is *not* one of equality, but is as 146 is to 100.

This ratio of forces holds good not only as regards a railway train rounding a curve, but also as regards planetary motions in their orbits; and the suggestions I have made regarding it do not, I think, in any way overestimate its vast importance.

Dr. Young's mechanical contrivance, alluded to in my above-mentioned letter, would tend to carry the bodies flung from it by centrifugal force (beyond the limits of his Whirling Table) the course which I ventured to suggest in connection with the sling: namely, not along the tangent, but along a line diverging about 40° from the tangent. I made some experiments which gave a divergence of about 30°, the difference being apparently due to friction; and I this afternoon arranged some mechanism which I hoped would make the friction practically almost a negligible quantity (as I believe it is in Dr. Young's mechanism), but my mechanism was not strong enough for the test, and gave way; so the angle has not been perfectly demonstrated by experiment, but the evidence is sufficient to justify a repetition of my request for the point to be demonstrated in one of our laboratories.

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

To Sir William Crookes, D.Sc., F.R.S., Secretary, the Royal Institution.

7th December, 1906.

Dear Sir,—As, in my letter of the 1st instant, I ventured to ask you kindly to have made in one of our laboratories the experiment which I had not succeeded in bringing to a clear issue on that afternoon, I think it right to advise you that I have since succeeded in effecting it, and the rough lines showed the angle of divergence from the tangent to be 47 to 48°.

That angle I take to result from the fact that the motion of the body along the revolving radius of the table is one of equable acceleration; so that, if started from a position placing its centre of gravity in the axis of rotation, it would, on reaching the periphery of the Whirling Table, have radial and tangental momentum in the ratio of 200 to 121.

That, by theory, should give an angle of about 59°; and the difference may be accounted for partly by friction, but chiefly by the practical difficulty of starting the body with its centre of gravity in the axis of the Whirling Table's rotation.

It seems to me that in a well-arranged experiment there would be no friction, but merely hammering action on the body as it traverses the revolving radius.

If you make the experiment I shall be much obliged if you would kindly let me know the result.

I am, dear Sir,
Yours truly,
WM. LEIGHTON JORDAN.

To Sir William Crookes, D.Sc., F.R.S., Secretary, the Royal Institution.

8th June, 1907.

Dear Sir,—Sir James Dewar's lecture last evening was splendid; it eclipsed all that I had before heard from him.

Of course he knew that the experiments he showed us were of first-class importance; but if he had shown the slightest consciousness of the fact that before he was half through the lecture the Vibration Theory of Light was dead; and that in the exquisitely managed experiment with the vapour of mercury he was merely slaying the slain, I should not be writing to you on the subject.

It was also clear that neither Lord Kelvin nor you (or many other mighty listeners) recognised what a great epoch-making event in the history of Science had occurred.

This could not have been so if it were not that the slaughter was indirect. *A new* and *the true* theory shone through the experiments; and the Vibration Theory peacefully vanished in its presence.

Of course we all know that the attempt made last Session to revive the Corpuscular Theory merely resulted in showing it to be doubly dead; but who then dreamed that the Vibration Theory was so soon to follow its rival to the limbo of exploded fancies!

I am, dear Sir,
Yours truly,
WM. LEIGHTON JORDAN.

To Sir William Crookes, D.Sc., F.R.S., Secretary, the Royal Institution.

10th June, 1907.

Dear Sir,—As I walked along Albemarle and St. James's Streets just after Sir William White's lecture had closed the Session of the Royal Institution on Saturday, a cloud of history rose around me. I realised what a triumph Sir James Dewar's lecture of the previous evening was for Sir Isaac Newton, whose sturdy thought had firmly kept him from ever accepting the Vibration Theory of Light, even when he became shaky as regards his Corpuscular Theory. I reflected also that it was the restless amateur Rumford, one of our enterprising American colonists, who founded and organised the Institution which had made such a lecture as that of Friday evening possible. The doors of that Institution, founded by the amateur Rumford for the free consideration of all questions connected with Natural Philosophy, are now slammed in the face of any criticism of the so-called First Law of Motion, which was in fact relegated to the limbo of exploded fancies more than forty years ago. I never had any special spite against that law; it was a clever and simple invention, which served the purpose of those who invented it, and is also the basis of a vast amount of the most abstruse mathematical demonstrations published by leading scientific societies. I had no special wish to disturb its proud position; I chanced, as an independent student of Natural Philosophy, not cramming to pass an Examination, but studying to arrive at the truth for my own satisfaction, and unhampered by the necessity of keeping in line with all that had come to be looked on as established principles in science:—in that freedom I recognised the existence of the innumerable forces of

tension caused by the connecting links of gravitation reaching from star to star, and forming by its multitude of lines of force everywhere a plenum of interacting forces which makes the supposed action of the so-called First Law impossible throughout the material universe.

I will now place before you only one field of action in which that discovery of the action of astral gravitation has had practical effect.

Sir George Gabriel Stokes, William Froude, and some others, not only scouted my attack on the 'First Law,' but woke up to the fact that it had never been given its due consideration as a practical law in terrestrial physics. They made up their minds that nobody had ever before understood the theoretical principles under which a ship should be constructed; and Stokes directed his unrivalled mathematical skill to show what modifications of accepted ideas of shipbuilding were required by that so-called fundamental principle of physics.

For a long time I really did not know what was going on; but after I became aware that the so-called First Law was being given practical application in the construction of our battleships and cruisers, I quickly found that erroneous principles had resulted in giving our ships too deep a displacement. I opened fire on our Naval Constructors through the Council of the Institution of Civil Engineers, partly because Sir William White, late Chief Constructor of the Navy, was a member of the Council, and partly because, as an Associate, I considered myself entitled to a hearing in that Institution.

About two years afterwards the keel was laid for what became known as the 'Secret Ship,' because, though avowedly to have great novelty in construction, the character of the change was kept dark. In about another year she was launched as the 'Dreadnought,' and it then

appeared that, though about 2,000 tons *heavier* than the heaviest of the battleships we had previously built, she drew about two feet *less*.

Her superiority as a seagoing ship is undisputed.

If, Sir William Crookes, you will exert yourself to think a little over that, you cannot fail to recognise that every battleship we had in commission just before the 'Dreadnought' was designed constitutes a practical demonstration to the effect that the so-called First Law of Motion is not true to nature. How much longer will you and other survivors of our generation in science allow yourselves to appear to continue to be supporters of that glaring error?

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

APPENDIX I. NOTE TO PAGE 3.

M V^2 AS CENTRIFUGAL FORCE IN PLANETARY ORBITS.

To Dr. J. H. T. Tudsbery, Secretary, the Institution of Civil Engineers.

21st November, 1903.

Dear Sir,—Though the object of my letter of 3rd July was merely to support my argument to the effect that the figure 32·2 used in all engineering text-books as the Foot-Pound-Second unit of gravity is erroneous and creates unnecessary complications in engineering formulæ, and that the true figure is 16·1; that letter constitutes (by pointing out the interaction of the formulæ I had been using) the first announcement of one of the most startling and majestic among the discoveries that have been recorded in the history of science. It appears to me that the fact of my argument having led to the declaration that a transverse action of the Earth's gravitation created by its rotation must beyond the moon's orbit become uncontrollable by the radial force in which it originates, has appeared to the Council so incredible as to lead them to consider the whole argument unworthy of serious consideration.

Stokes and Huxley were, I am sure, thoroughly honest in their opposition to my views in the years 1866–1868, and were probably influenced even more by my repudiation of the doctrine of the 'Indestructibility of Matter' than by my attack on the 'First Law of Motion'; but the Royal Society, which opposed me then, has now adopted that main point, and are disparaging me now in connection with my further development of that new philosophy exactly as Huxley, Stokes, and others treated the first step as too glaringly 'on the wrong track' to be for a moment entertained by anyone who understood what were accepted as fundamental principles in scientific knowledge.

The point now at issue is so thoroughly interwoven with formulæ in engineering text-books that, in duty to myself, to the Council, and to Engineering Science, I urge some further evidence in reinforcement of my argument as to 16·1 being the true foot-pound-second unit of gravity.

In letters on the theory of the Pendulum, addressed to the Royal Society, I some time ago pointed out that the normal length for a Seconds Pendulum vibrating 180°, where gravity gives a fall of 16·1 feet per second, is 3·22 feet; because the work done by gravity on the pendulum in the second is naturally divisible into five equal parts, of which the fall through the length of the radius is one part.

I now urge on the Council the following figures :—

The radius of the moon's orbit is 1,261,000,000 feet, which ÷ 3·22 (normal radius of Seconds Pendulum)=391,600,000; and the Square Root of the latter figure gives the moon's corresponding period as 19,790. That figure × 60 (the moon's distance in units of the Earth's radius)=1,187,400; which, divided into the length of 180° of the moon's orbit (3,959,500,000 feet) =3,334 feet per second :—which is in fact the moon's velocity.[1] I wish, first, to urge that the above connecting link between the radius of the pendulum and that of the moon's orbit shows 16·1 to be the true foot-pound-second unit of gravity. The fall along the radius is $\frac{1}{5}$ of the 16·1 feet of work done by gravitation on the pendulum in the second; but as the moon falls only ·0044 of a foot per second, the velocity of 3,334 feet per second along the orbit practically represents the work done on the moon per second, though that fraction of a foot is theoretically included as part of the work done.

And, secondly, I beg to draw special attention to the above use of the figure 60 as a multiplier instead of 3,600 (=60^2).

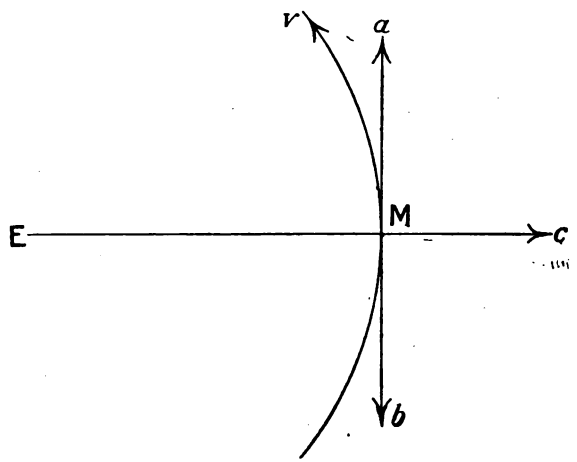
[1] The velocity of 3,334 feet per second agrees with that of the pendulum, because it is that which would result in the moon's orbit from the action of a transverse force equal to gravity. But as the transverse force acting on the moon in fact exceeds gravity as 146 is to 100, giving a transverse velocity exceeding radial velocity, 121 is to 100, the absolute velocity of the moon along its orbit round the common centre of gravity of the earth and moon is 3,704 feet per second.

The velocity of 3,334 feet per second which is at present treated in all astronomical works as the moon's *orbital* velocity, is in fact the velocity measured along the chord of an arc of 90° of its orbit.

The force of gravity is 3,600 times greater where acting on the pendulum than in the moon's orbit, and the use of that figure is demanded in any full theoretical statement. But the figure 60 gives a short cut to the true velocity in the moon's orbit because the error caused by its use is balanced by an equal error caused by the omission of the action of astral gravitation in the moon's orbit, constituting a resistance as the square of the velocity of motion.

The full theoretical statement of the case is as follows :—

The relative periods for the Seconds Pendulum and the moon (as Square Roots of radius) are 1 and 19,790 seconds respectively × inversely by the force of gravity in the respective



positions (as 1 is to 3,600), making the relative periods 1 second and 71,244,000 seconds respectively.

The latter figure divided into 3,959,500,000 feet (=180° in the moon's orbit) gives 55·58 feet per second as the velocity which would counterpoise gravity in the moon's orbit if the Vis Viva of motion were not restrained by the reaction of astral gravitation against it.

If the earth and moon were a solitary system, the tangental force, M *a*, of a velocity of 55·58 feet per second, M V, would counterpoise the reciprocal force of gravitation E M.

But the reaction, M *b*, of astral gravitation against the motion enables gravity to control a greater velocity than it could other-

wise do; and I have elsewhere shown that by that action the controllable velocity is increased in proportion as the distance is increased; so that, instead of the normal velocity of 55·58 feet, the velocity requisite to counterpoise gravity in the moon's orbit becomes $55{\cdot}58 \times 60 = 8{,}334$ feet per second.

That lifting of velocities above their normal distance from the sources of any transverse action of gravitation is the foundation of the startling law which I announced in my letter of 3rd July last, to the effect that some distance beyond the orbit of Neptune the transverse action of the sun's gravitation becomes uncontrollable by the radial force in which it originates.

The reciprocal force, E M, between the earth and moon, actually does counterpoise the force M c; and if the velocity were not lifted, by reaction, above its normal distance, then, to whatever extent the reciprocal radial force might be decreased by increase of distance, there would remain the same relative quantity to represent the square of the orbital velocity; and the latter would, therefore, be equally controlled at all distances.[1]

Be the realities of the ratios in that vast interaction of forces, however, what they may, the figures which connect the work done by gravitation on the pendulum with that done on the moon must, I think, be admitted as a proof that 16·1 is the true Foot-Pound-Second unit of Force in Gravitation measure.

I am, dear Sir,
Yours truly,
WM. LEIGHTON JORDAN,
Assoc. Inst. C. E.

[1] In a railway train the relative amounts of centrifugal force on curves of different radii are as $\frac{v^2}{r}$, as in the experiments alluded to in my letter of 30th June, 1905 (*see* p. 12).

In a paper on 'The Scrapping of the First Law of Motion' which I have presented to the Institution of Civil Engineers, I have pointed out that in Astronomical phenomena such as the earth's orbital motion, the velocity is due to a combination of the tangental and centripetal forces; whereas on the railway curve the centripetal force reduces, instead of increasing, the tangental velocity.

To Dr. J. H. T. Tudsbery, Secretary, the Institution of Civil Engineers.

18th January, 1906.

Dear Sir,—I beg that you will kindly bring before the Council of our Institution the following set of formulæ for calculations connected with gravity, in correction of an absolute error which at present pervades all engineering text-books and necessitates unnecessary complications in formulæ for gravity.

'Molesworth's Pocket Book of Engineering Formulæ,' p. 362, 23rd edition, takes:—

N = Number of seconds;
S = Space fallen through in feet;
V = Velocity in feet per second, required in N seconds or S space;

and on that data gives the following formulæ:—

$$V = N \times 32{\cdot}2$$
$$V = \sqrt{S \times 64{\cdot}4} = 8{\cdot}025\sqrt{S}$$
$$S = N^2 \times 16{\cdot}1$$

Formulæ are thus given for only three out of ten different cases for which a formula may be wanted.

To get a complete systematic set of formulæ with g as the basis of them all, it is necessary to substitute *mean velocity* for 'velocity' in the above data, and then we get the following:—

$$V = N \times g$$
$$S = N^2 \times g$$
$$S = V^2 \div g$$
$$N = V \div g$$
$$N = \sqrt{S \div g}$$
$$V = \sqrt{S \times g}$$

Adding to Molesworth's above data:—

W = weight in pounds,
M = momentum, and
K = Vis Viva in units of force = 1 pound weight;

then :—

$$\left.\begin{aligned} M &= N \times g \times W \times 2 \\ M &= \sqrt{S \times g} \times W \times 2 \cdot \\ K &= (N \times g \times W \times 2)^2 \\ K &= (\sqrt{S \times g} \times W \times 2)^2 \end{aligned}\right\}^*$$

* or, $K = M^2$.

The error which has to be corrected in order to get a complete set of formulæ with the same figures systematically representing gravity, is to be found on Molesworth's page 363, where it is said that :—

'g = force of gravity, feet per second.
'g = 32·1889 at London at the level of the sea.'

The fact is that if g = the force of gravity in feet per second, then g (instead of 32·1889) = 16·094445 at London at the level of the sea ; or, for all ordinary calculations, 16·1 is a sufficiently close approximation, and correctly represents g in each of the above ten formulæ.

The fact that Molesworth has to change his figures in each of his three formulæ shows that he starts on a false basis by taking g = 32·2.[1]

I am, dear Sir,
Yours truly,
WM. LEIGHTON JORDAN.

[1] The origin of this error is pointed out in my 'Essays on the Action of Astral Gravitation in Natural Phenomena,' published in the year 1900.

APPENDIX II. NOTE TO PAGE 12.

THE SLING.

To Dr. J. H. T. Tudsbery, Secretary, the Institution of Civil Engineers.

10th November, 1906.

Dear Sir,—Every Engineering Text-Book I have met with agrees in asserting, or taking it for granted, that a railway train leaving a curve under the action of centrifugal force tends to follow a tangent to the curve.

The Salisbury accident appears, however, to have led to a new consideration of the subject; as, in connection with it, the Editor of the *Engineer* says there is 'a very commonly held view that when a body flies outwards under the influence of centrifugal force, it moves radially away from the centre of gravity.' (5th October, 1906.)

I never before heard such an effect as above mentioned suggested in connection with the motion of a railway train.

What I have argued in connection with the subject is that D'Alembert's so-called fictitious force ($\frac{v^2}{r}$ acting radially outwards) is not a 'fictitious,' but a real force.

That is, however, only one component of the force acting on the train as it leaves the rails; and I have suggested that the combination of that force with the *vis viva* of tangental momentum tends to carry the train away from the curve at an angle of about 41° (40·724+) from the tangent (49·276— from the radius vector at the moment of deviation).[1]

[1] The experiments referred to in the next letter show the above angle to be that at which the released body enters the tangent from the curve, and that angle is a consequence of the ratio of tangental to radial force, being as 146 is to 100. To illustrate this, let a motive force tend to give a mean velocity of 1,000 feet per second from B to A in the diagram

I have submitted to you the result of experiments which appear roughly to confirm that theoretical action. The question arose incidentally in connection with my demonstration of the fact that Engineering Text-Books are all wrong in supposing 32·2 to be the Foot-Pound-Second equivalent of gravity. That demonstration (commenced in the work I published in the year 1900 and culminating in the letter I addressed to you on the 18th January last) entitles the collateral suggestions I have made to respectful consideration by serious men holding influential and responsible public positions in the Engineering profession.

The published proceedings of our Institution abound with cases in which diametrically opposite views are expressed in argument, and, therefore, in just so many downright blunders which have helped to show the way to truth; and I appeal to the Council to have the above angle of 41° openly tested by more reliable experiments than I have been able to arrange.

The fact that 16·1 (not 32·2) is the Foot-Pound-Second equivalent of gravity is no longer open to reasonable dispute; and if practical experiment should show that I am mistaken in

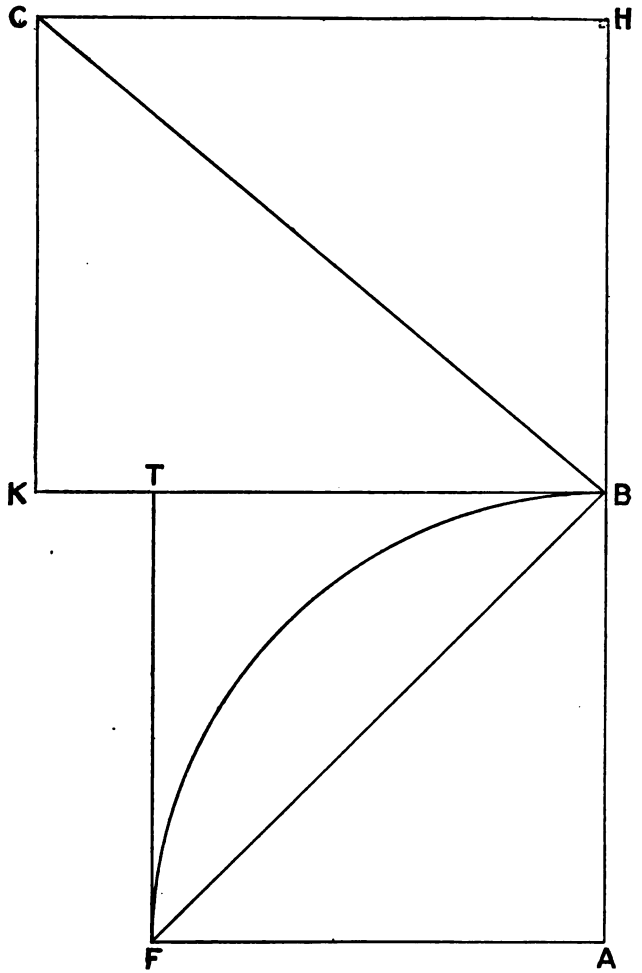
on page 59. An equal force acting from B to T, in combination with the above, would give a resultant of 1,414 feet per second along the diagonal, B F, of the square B T F A. To traverse the quadrant B F instead of the diagonal in the same time a velocity of 1,570 feet per second is requisite.

Let the force B A act from B towards H instead of towards A. Then, to give a resultant of 1,570 feet per second along the diagonal B C, the force acting along B T must be to that in the line B H in the ratio of 146 to 100; tending to give a velocity of 1,210 feet per second along B T.

Extend B T to K, making the ratio of B K to B T as 1,210 to 1,000, and complete the parallelogram K C H B. The diagonal C B then divides the right angle at B into the two angles 40·724+ and 49·276− alluded to above, and it is the line in which I supposed the existence of a tendency to deviate from the tangent. It, however, in fact represents a succession of instantaneous lines of pressure from the curve to the tangent.

The fact of a body describing a quadrant of 1,570 feet instead of an equal distance along a diagonal line, shows that the forces B A and B K are not independent of each other; and I have shown that, in astronomical phenomena, whereas a direct action of gravitation at B from A is inversely as the *square* of the distance, any transverse action the same force may exert is inversely as the *cube* of the distance. And, therefore, any tendency to deviate outwards from the circle is corrected by a *relative decrease* of transverse action; and any tendency to deviate inwards is corrected by a *relative increase* of transverse action.

supposing that the train tends to spring away at an angle of 41° from the tangent, that mistake will remain overshadowed by my correction of the recently standing error as regards the equivalent of gravity.



Major Pringle, in his report on the Salisbury accident, says nothing as to any evidence bearing on the above question: perhaps because he did not consider the question appropriate for

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his report: perhaps because he considers that every well-educated engineer *knows* perfectly well that the train does in fact tend to deviate along the tangent, and that it would be useless to discuss the complications of evidence in that case. By having a simple experiment arranged on a Whirling Table, the Council can for ever set at rest the difference of opinion alluded to, as above stated, by the Editor of the *Engineer*.

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

To Dr. J. H. T. Tudsbery, Secretary, the Institution of Civil Engineers.

14th December, 1906.

My dear Sir,—Ever since replying, on 16th November, to your kind note of 14th *idem* I have been engaged in preparing a paper on the so-called First Law of Motion. I find it, however, expedient, as a preliminary step, to ask indulgence to allow me to correct an error in a letter I addressed to you on 25th October, 1905.

The preparation of my paper made it essential for me to make some mechanical experiments for the purpose of testing the sling action dealt with in my above-mentioned letter; and I find that I had misunderstood the rough experiments mentioned in that letter. In the ordinary sling the plummet, on being released, follows the tangent to the curve in which it had been revolved. To get a deviation outwards from that tangent it is necessary to have a solid radius instead of a string, and to release the plummet, not at the periphery of the circle but anywhere between that and the centre. As the distance of release from the periphery is increased, the angle of deviation from the tangent increases up to a maximum of about 59°.

In the ocean the water arrives with more or less of radial momentum at the successive circles it crosses, and must therefore tend to spring from the tangent in the manner shown by Diagram IV. of my above-mentioned letter. The fact that more than merely tangental momentum seemed requisite to account for some phenomena of oceanic circulation forced itself on me many years ago; but I have never, until making my

recent experiments, realised the simple character of that action, though it is virtually explained in detail in papers which I submitted to the Council of the Royal Geographical Society in the year 1876. And also, in the 'Treatise on Vis Inertiæ,' which I published in the year 1868, I commented on the intrinsic difference between the action of forces in ordinary sling experiments and that of forces acting on drops of water in the ocean and in astronomical phenomena. Those views are confirmed by my recent experiments, and make my recent mistake regarding the ordinary sling doubly strange.

I am, my dear Sir,

Yours very truly,

WM. LEIGHTON JORDAN.

APPENDIX III. NOTE TO PAGE 17.

THE MOON'S PERIOD OF REVOLUTION.

To Dr. J. H. T. Tudsbery, Secretary, the Institution of Civil Engineers.

13th November, 1905.

Dear Sir,—In my letter of 6th instant I turned aside from the majestic subject of Stellar Time to that of your letter of 4th ultimo, which required attention; and I now revert to the former for the purpose of commenting on a modification of hitherto accepted ideas as regards lunar and planetary velocities of orbital motion of such magnitude as glaringly to demand explanation.

The period of 24·38 days, which I have given as that in which the moon completes an orbital revolution, requires (the radius of the orbit being $1{,}245 \times 10^6$ feet) a velocity of motion along the line of the orbit at the rate of 3,714 feet per second, whereas astronomers have been agreed in treating the actual velocity measured from star to star as only 3,334.

That apparent discrepancy of 380 feet per second is approximately due to the fact that astronomers measure the velocity *not* along the line of the orbit actually traversed by the moon, but along successive chords.

As Sir Isaac Newton's demonstration of the action of the earth's gravity in the moon's orbit is the keystone of Natural Philosophy, when extended beyond terrestrial regions, I give the following statement to show the approximate agreement of his figures with the period of 24·38 days which I have given for the moon's orbital revolution.

Chambers gives 3,334 feet per second as the moon's orbital velocity, which (though not stated to be so in astronomical works) is the distance measured from star to star along the

chord of the arc traversed by the moon; and not along the orbit, which is the moon's true path.

That figure, 3,334 being the mean velocity along the chord makes the corresponding mean velocity along the radius of the orbit 3,334 divided by 1·414=2,357·8 feet per second.

Newton, in the 'Principia,' gave for the moon's fall from the tangent to its orbit, 15 feet 1 inch $1\frac{4}{9}$ lines per minute in old French measure; which I make approximately equivalent to ·00447 of an English foot per second.

That figure, ·00447, divided into 2,357·8 as the mean velocity of fall along the radius in an arc of 90°, obtained as above from Chambers's orbital velocity of 3,334 feet per second, gives 527,472 seconds as the time in which the moon falls the length of the radius of its orbit; and, therefore, makes the period in which it completes an orbital revolution 527,472 × 4 =2,109·888 seconds, or 24·42 days. The discrepancy between 24·38 and 24·42 is partly due to the difference in the estimates of the moon's distance, which were accepted by Newton and by Chambers respectively.[1]

I am, dear Sir,
Yours truly,
WM. LEIGHTON JORDAN.

[1] A more definite estimate is given in my letter of 3rd May, 1906, on page 32, but is subject to slight corrections.

APPENDIX IV. NOTE TO PAGE 25.

MR. FRANCIS DARWIN ON 'THE UPWARD FLOW OF SAP IN PLANTS.'

To Dr. J. H. T. Tudsbery, Secretary, the Institution of Civil Engineers.

4th February, 1905.

Dear Sir,—Fluid motion has been so much before our Institution in recent discussions as to make it specially appropriate for me, in my endeavour to induce our Council to rescue the Royal Society from its bondage to obsolete notions on the subject, to refer to a lecture on 'Blood Pressure in Man,' delivered in this Institution last evening.

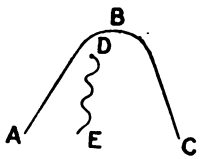
In the course of his lucid and very interesting treatment of the subject, the lecturer described an enormous frictional resistance to the circulation in the arteries and veins just as a practical engineer would describe the resistance to the flow of water through a pipe under pressure from a hydraulic ram, and commented on the great contractile power of the heart requisite to overcome such resistance.

I do not, in the absolute manner in which I deny the existence of the Great Southern Ocean tide which is supposed to be the origin of the tides all over the world, deny that the heart can exert the supposed amount of pressure; but wish to show reason for considering that the motive force which keeps up the circulation acts locally throughout the course of circulation requiring the heart to exert force proportional only to the relative quantity of the blood it may contain; and if called on for more than that exertion, it is then struggling against a diseased condition of the system.

Let A B C be a mountain with a river, D E, running down its side from a spring at D.

The water is raised, within the mountain, to D by atomic or molecular gravitation preponderating over gravity. But when the water reaches the orifice at D, gravity preponderates and carries the water downwards; the molecular gravitation over which gravity preponderates being represented by the friction which checks the downward motion.

It seems to me that the circulation of the blood must be assisted by a similar action of molecular gravitation between the molecules of the blood and the sides of the narrowing arteries; and in the widening veins the capillary action is



diminished, leaving the molecular gravitation of the blood itself to draw it together and into the heart, where only it can aggregate, just as moisture on a leaf is drawn into the formation of a dewdrop in opposition to the action of gravity. Molecular gravitation appears to be a motive force equally distributed throughout the circulation in conditions of health, and the question at issue is as to the ratio it bears to the hydraulic ram or 'head of water' pressure caused by the contractile action of the heart.

I am, dear Sir,
Yours truly,
WM. LEIGHTON JORDAN.

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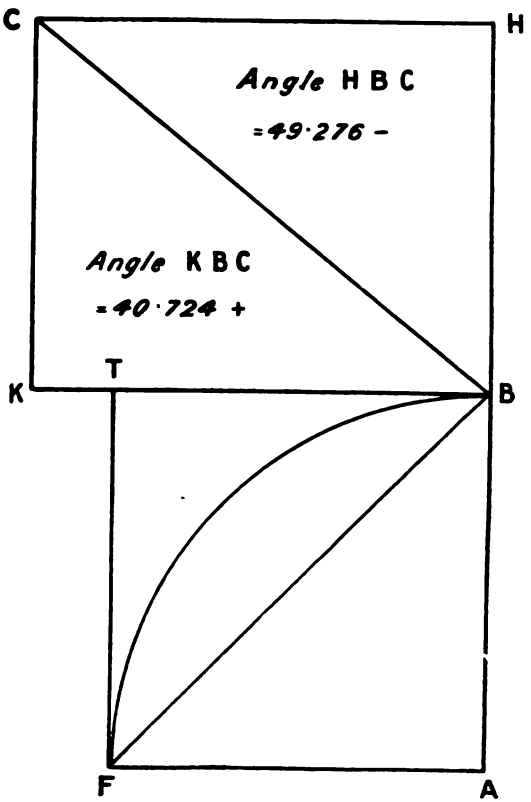
REMARKS IN CONNECTION WITH

LECTURES DELIVERED IN THE ROYAL INSTITUTION

AND IN

THE INSTITUTION OF CIVIL ENGINEERS

DIAGRAM I.



THE SLING FORCES

The ratio of the tangential force, B K^2, to the radial force, B A^2, is nearly $14\frac{2}{3}$ to 10. If arithmetic could give that ratio accurately it would also give the ratio of the circumference of the circle to the radius.

THE SLING

PART II

REMARKS IN CONNECTION WITH
LECTURES DELIVERED IN THE ROYAL INSTITUTION
AND IN
THE INSTITUTION OF CIVIL ENGINEERS

BY

WM. LEIGHTON JORDAN

MEMBER OF THE ROYAL INSTITUTION; ASSOCIATE OF THE INSTITUTION OF CIVIL ENGINEERS; FELLOW OF THE ROYAL GEOGRAPHICAL SOCIETY, OF THE SOCIETY OF ARTS, OF THE ROYAL STATISTICAL SOCIETY, AND OF THE ROYAL METEOROLOGICAL SOCIETY; PAST PRESIDENT OF THE ENGLISH LITERARY SOCIETY OF BUENOS AYRES

LONDON
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THE FIRST PART

PREFACE

As a connecting link between this Volume and its first part I have reproduced, as a Frontispiece, a diagram showing the ratio of tangential to centripetal force in all circular motion.

A B T F being a square, A B^2+B T^2 is equal to the diagonal B F^2.

Also, the side B H of the parallelogram B H C K being equal to A B and in the same straight line, then A B^2+B K^2 is equal to the diagonal B C^2, whatever may be the length of B K.

Let a motive force tend to give a mean velocity of 1,000 feet per second from B to A. An equal force acting from B to T in combination with the above would give a resultant of 1,414 feet per second along the diagonal B F of the square B T F A. To traverse the quadrant B F instead of the diagonal in the same time, a velocity of 1,570 feet per second is requisite.

Let the force B A act from B towards H instead of towards A. Then, to give a resultant of 1,570 feet per second along the diagonal B C, the force acting along B T must be to that in the line B H in the ratio of 146 to 100; tending to give a velocity of 1,210 feet per second along B T.

Extend B T to K, making to ratio of B K to B T as 1210 to 1000, and complete the parallelogram K C H B. Then the quadrant B F is equal to the diagonal B C, because they are resultants of similar forces, B K^2+B H^2 and B K^2+B A^2. And the sides of the square described on B C are together equal to the circumference of the circle of which the curve line B F is a quadrant.[1]

[1] The moon, and each planet, follows the curve B F, because the transverse force B K^2 in its orbit is a function of the centripetal force B A^2, decreasing as the *cube* of the distance from A increases, whilst the centripetal force itself decreases as the *square*; and, therefore, any tendency to deviate outwards from the circle is corrected by a relative decrease of transverse action, and any tendency to deviate inwards is corrected by a relative decrease of centripetal action. The action of each perturbing force in the orbit is either corrected, or else given permanently a natural limit in that same manner.

Arithmetical numbers will not express precisely the above ratio of forces—the magnitudes at which the two angles (together 90°) specified in the Frontispiece make the diagonal B C exactly equal to the quadrant B F—or the ratio which those two equal lines bear to the two equal lines B H and B A; and all the figures given above are therefore merely the approximations given by integral numbers on the basis of 1000 representing a side of the square A B T F.

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LETTERS

To Sir William Crookes, D.Sc., F.R.S., Secretary, the Royal Institution.

24th April, 1908.

Dear Sir,—In a letter which I addressed to you on 8th June, 1907, I endeavoured to interest the Royal Institution in the announcement I was just then enabled to make of a discovery which I had long ago vaguely imagined, notwithstanding what seemed to be an insuperable barrier of plausible and generally accepted argument which had appeared to be at variance with it. In that letter I limited myself to the declaration that the vibration theory of light which had long been accepted in the scientific world had unconsciously been exploded by Sir James Dewar in the course of his lecture of the previous evening.

The idea that light requires the assistance of Æther, 'the ether,' or that of any substance to enable it to vibrate may be relegated to the limbo of exploded fancies. Though light originates in transmutation from material substance, it is, when once created, an independent immaterial force which emanates in all directions from the source of its creation; and it vibrates, without extraneous assistance, in its own natural course of filling space, expanding as the cube of the distance traversed; having, therefore, as much, or more, lateral as direct motion. It requires no help from any material substance to enable it to follow its course. Only in empty space it has perfect freedom of action, and it is

merely more or less hampered, distorted, or spent by encountering any substance, however attenuated.

The abolition of the idea of any material substance being necessarily distributed throughout interplanetary space to account for the vibration of light constitutes a step of far-reaching importance in natural philosophy. In the action of gravitation there is no such lateral clashing of the force.

The force of gravitation which is in fact exerted by every material particle would be non-existent outside any particle but for the existence of surrounding particles, with each one of which gravitation creates for it a reciprocal bond of union.

The action of gravitation is in lines of force extending from every material particle to every other particle, and differs in that respect from the action of any such independent force as light, as it does not spread itself for the purpose of filling space, but exists only where some material particle exists to reciprocate the action of other particles. Light, on the contrary, is independent of such reciprocal action, and spreads itself to fill its sphere of action without requiring a reciprocal bond of union to give it existence at any point within that sphere.

Perhaps you will not fully appreciate the importance of that distinction without knowledge of what I published on the 'creation of force' forty years ago. The arguments I then used have in fact led to the elucidation of this practical difference between the modes of action by which light and gravitation are respectively distributed around any centre from which they may respectively act.

That difference may appear superficially to be a trivial distinction, but it is in fact a point of most profound and far-reaching importance in the elementary principles of natural philosophy.

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

To Sir William Crookes, D.Sc., F.R.S., Secretary, the Royal Institution.

6th May, 1908.

Dear Sir,—The 'Scientific Work of Lord Kelvin' must, I think, be considered by all members of the Institution as the most appropriate subject with which the current session of evening meetings could have been commenced; and the manner in which it was dealt with by Dr. Larmor last Friday was certainly of intense interest to me as one item in the Lecture Hall where all were keen to hear the achievements of the most renowned of our generation of scientific men officially summarised by the Secretary of the Royal Society.

Lord Kelvin may fairly be regarded as a central figure in a half-century which may perhaps hereafter take its place in history as the most remarkable transition period which the scientific world has hitherto experienced. It commences with the work of John Dalton, well established for its basis, and closes with the startling discovery of the action of radium; and, as the dust of atomic theories has to some extent led to a confused treatment of fundamental principles of natural philosophy, I think it expedient to press on the Royal Institution some important points which have not been recorded by Dr. Larmor.

Lord Kelvin was in his younger days author, jointly with Professor P. G. Tait, of a treatise on Natural Philosophy, which soon became the most generally received text-book on the subject. In the Preface to that treatise, dated July 1867, the authors say:—

'We give Newton's "Laws of Motion" in his own words, and with some of his own commentaries—every attempt that has yet been made to supersede them having ended in utter failure. Perhaps nothing so simple, and at the same time so comprehensive, has ever been given as the foundation of a system in any of the sciences.'

I have been specially interested in the opinion then held by Lord Kelvin and Professor Tait, because in 'An Investigation of the Forces which determine the Position and Movements of the Ocean and Atmosphere,' which I had published in the year 1866, I had substituted for Newton's 'First Law of Motion' a new definition of his 'Centrifugal Force,' and the above extract from the first edition of the treatise by Thomson and Tait showed their rejection of my new view regarding the action of astral gravitation.

It is only because (as just above suggested) in the dust of atomic theories broad principles have been almost lost sight of, leading scientific men have remained unconscious of the fact that the opinion so clearly and so forcibly expressed by Thomson and Tait in 1867 has been practically abandoned throughout the scientific world. That change is made evident by the article 'Mechanics,' in the ninth edition of the 'Encyclopædia Britannica,' published in 1883. Professor Tait there says :—

'The first law is superfluous, because its consequences are all implied (by negation) in the statement of the second.'

That shows a decided change of front between the year 1867 and the year 1883 as regards the merits of Newton's mode of stating the Laws of Motion. I must, however, frankly say that Professor Tait's statement to the effect that Newton's First Law is implied in his Second Law passes my comprehension; and I leave its explanation to anyone who finds himself able to understand it. All I know is that my substitution of a transverse action of Newton's law of gravitation to do all the work in the solar system for which he supposed his first law of motion to be requisite does not invalidate his second and third laws. Newton's law of gravitation supplies a transverse action in each planetary orbit, the ratio of which to the radial action is, in each orbit, 46 is to $\frac{100}{100}$ approximately; and Tait is therefore quite

right in saying that the first law is superfluous; but I find no reasonable foundation for the idea of its being implied in the second law.

The 'Astronomical Register' in August 1874 concluded a review of one of my books with the words: *Nolumus leges motus mutari.* What have they—and you, Sir William Crookes—to say to the ninth edition of the 'Encyclopædia Britannica'? Tait was chosen to deal with the subject in that publication as being, with Lord Kelvin behind him, the most generally accepted authority. Do you still stand in the position held by the 'Astronomical Register' thirty-four years ago, blind to its unfair persistence in hostility to astral gravitation?

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

To Sir William Crookes, D.Sc., F.R.S., Secretary, the Royal Institution.

8th May, 1908.

Dear Sir,—The second point contemplated in my letter of 6th instant is created by Dr. Larmor's comments on the recent application of gyroscopic action by Dr. Schlick to the prevention of the rolling motion of ships at sea.

Dr. Schlick's work was alluded to after an interesting account of Lord Kelvin's gyroscopic experiments as if the action had been understood and its application as contrived by Dr. Schlick in fact foreseen by Lord Kelvin. Dr. Larmor's statements to the above effect show a thorough misappreciation of what has been done by Dr. Schlick. In describing Lord Kelvin's work Dr. Larmor did not exhibit or allude to any experiment bearing in any manner on the main principle involved in Dr. Schlick's work. If either the theoretical ideas or their practical application had ever

before been described I could scarcely have escaped hearing something of them; in fact, however, I went to Dr. Wurl's recent lecture on the subject at the Society of Arts puzzled by what I had previously heard, and I left the Lecture Hall more bewildered than I had entered it. But before I had walked for many minutes along the Strand, thinking over what I had seen, I grasped the simple idea involved and its clever application.

It has now for many generations been a commonplace proceeding among civilised people to arrange a weight in a grandfather's clock in such a manner that, instead of falling freely so as to give with half a second's work a heavy blow, a brake is applied to the motion so as to equally distribute the force over seven or eight days instead of allowing it to expend itself in half a second; but it seems to me that until after Dr. Schlick had not only conceived the idea of putting a brake on the instantaneous gyroscopic action which if left free would increase the rolling which brought it into play, but had also made it experimentally effective, neither Lord Kelvin nor any other scientist had done anything in the matter either in theory or in practice; at any rate, Dr. Larmor showed no experiment touching what seems to me to be Dr. Schlick's completely new invention.

Thomson and Tait's ideas at the date of the experiments and writings dealt with by Dr. Larmor are now quite obsolete as regards gyroscopic action; they neither of them then understood its fundamental principles, and I have seen no indication of either of them having done so since. Be that, however, as it may, I am sure Lord Kelvin would repudiate the idea of Dr. Schlick's special discovery having been anticipated by him, and his renown is great enough without taking what belongs to other men.

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

To Sir William Crookes, D.Sc., F.R.S., Secretary, the Royal Institution.

23rd May, 1908.

Dear Sir,—'He did not say what the Ether is, did he?'

'No; *that* he did *not* tell us.'

The above words I chanced to overhear just before last night's lecture at the Royal Institution, and have no doubt they referred to Professor Trouton's very interesting lectures entitled, 'Why Light is Believed to be a Vibration' (12th instant), and 'What it is which Vibrates' (19th instant).

I watched carefully as Professor Trouton repeated one by one all the most important of the experiments which the marvellous resources of the Institution have accumulated for the purpose of analysing the action of light; and after the first lecture it seemed to me that the lecturer could scarcely any longer imagine that light requires the assistance of any material substance to enable it to travel from the source of its creation. I was therefore disappointed when the only conclusion arrived at was that the *same ether* which propagates light by its vibration, suffices also to account for electrical action; but I found two consolations—one in the fact that the corpuscular theory was ignored as if no longer running as a competitor for the explanation of light; and the other in the fact that *one* ether only is now wanted by science, instead of two, or three, or four, or five, as have from time to time been theoretically created, each, for its own purpose, filling the universe and playing its own game in every point of space regardless of opposite actions at the same points by its co-existent ethers.

I did not grasp any intrinsic divergence by Professor Trouton from Maxwell's electro-magnetic theory of light, but his general method of dealing with the subject seemed to me to constitute a step in the right direction as facilitating

a transition to the idea that light spreads, and vibrates as it does so, by its own inherent action; and that material substances all more or less thwart or distort its course; variously separating, transmitting, reflecting and deflecting its rays.

You know, Sir William Crookes, that Science once kept a tortoise to carry the earth and keep it from tumbling nobody knew where; is the Royal Institution to persist in supposing that light requires 'the ether' to carry it any more than the earth requires the tortoise?

The above is in some respects a suitable prelude to last night's lecture. It was a treat to follow the masterly manner in which the lecturer handled his majestic subject and arrived at his provisional estimate of the diameter of the material universe as 30,000 light years.

In the course of the lecture I reflected that it is now much more than forty years since I first read the lemmas prefixed to Newton's 'Principia,' and put them in quarantine; more especially Lemma II, as the keystone of their argument; and a few years ago a matter which struck me as clearly at variance with that argument led me to look at it again, with the result that though I did not feel strong enough in mathematics, or in possession of leisure enough, to make a formal and open attack on it, I framed a proposition directly at variance with it which I addressed to the Royal Society on the ——.

I limit myself for the present to suggesting that the above-mentioned Lemma of Newton's is the basis of the persistence in an error arrived at by measuring the velocities in planetary orbits along chords of arcs from point to point instead of along the arcs actually traversed. And also that the reality of the absolute velocities of motion along the arcs is a strong corroboration of the opinion arrived at by Professor Kapteyn to the effect that the material universe is of a definitely limited extent.

My attention has been so much distracted from Professor Larmor's lecture that I fear some points on which I hoped to comment have slipped my memory, at least for the moment. I must, however, say that I have looked at the paper by Lord Kelvin in the 'Philosophical Magazine,' published since his death, and referred to by Professor Larmor. The point in the paper which struck me as of the greatest interest is the manner in which it concludes with a quotation, given in inverted commas to show it to be *verbatim*, from a lecture delivered by Lord Kelvin himself in the year 1854, making a suggestion to the effect that gravitation may be the antecedent of all the motion, heat, and light at present in the universe.

I felt specially interested in that last point touched on by Lord Kelvin in his final effort in the cause of science, because in the year 1866, when I published my first work, I supposed that idea to have been originated by Grove in his 'Correlation of Physical Forces,' to which I alluded in the Preface of the work published that year, and also in the Appendix of another work I published in 1868. The bringing forward of the quotation now, just fifty-four years after its publication, is also of still greater and general interest as showing the vast importance attached by Lord Kelvin to the idea he then expressed, which has certainly developed to an extent and in a manner unthought of by either Kelvin or Grove when they were its bold and far-sighted pioneers.

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

To Sir William Crookes, D.Sc., F.R.S., Secretary, the Royal Institution.

13th June, 1908.

Dear Sir,—On the 3rd June, 1904, you gave the members of the Royal Institution the pleasure of hearing Professor Svante Arrhenius lecture on ' Electrolytic Dissociation.'

He puzzled me on the main point notwithstanding all his interesting detail.

He began by telling us that modern science has reached the same conclusion as Democritus, who taught that ' bodies are built up of indefinitely small indivisible particles which he called atoms.' And he further told us that Newton accepted the atomic theory of Democritus.

It seems to me that there is nothing in the history of science that can be more certainly relied on than that Arrhenius is right in saying that Newton and Democritus fully accorded in considering material atoms to be indivisible particles. And if modern science stands with Democritus and Newton on that fundamental point, its position is very clearly defined by both those philosophers in unambiguous language. But, after all that, the lecturer concluded by virtually adopting the idea of matter being only a manifestation of ' electro-dynamic forces,' and declared that ' the bold previsions of Sir William Crookes seem to be rapidly acquiring a concrete form, to the great benefit of scientific evolution.'

I do not know what the bold previsions alluded to are. But it seems to me that the simplest of common sense suffices to make it evident that the idea of matter being merely a manifestation of ' electro-dynamic forces ' is fundamentally at variance with the ideas of Democritus and of Newton as to the eternal indivisibility of the atom. Modern science either considers every atom of the material universe to have existed from eternity and to be for ever indivisible, or else

it has abandoned the ideas of Democritus on that question. What, in connection with electrolytic dissociation, is there that is bold in the previsions of Sir William Crookes if they do not constitute some advance from Newton's idea that material atoms were 'in the first creation made so hard that they can never be broken in pieces'?

Is the electro-dynamic theory alluded to by Professor Arrhenius, and which I must suppose to be clearly understood by him and by its inventors, most in accordance with that idea of Newton's or with the diametrically opposite idea which I have propounded to the effect that in the transformation of water to steam by the action of heat the atoms which existed in the water are not merely *scattered*, but are each one individually rent asunder and transformed in a manner utterly at variance with the ideas held by Democritus and by Newton under any reasonably commonsense interpretation of the language used by either of those philosophers?

Where does Arrhenius place modern science now?

You, Sir William Crookes, must surely know what he means by his statements. Is modern science with me as regards the transformation of the atom, by dissociation of its constituent parts, as water is converted into steam by the application of heat? If so, then it has absolutely abandoned the atomic theory held by Democritus and by Newton.

Another puzzle in connection with this question is that if atomic dissociation results from mere change of temperature whenever any cook boils a kettle, why should that be called 'electrolytic dissociation'? The dissociation has no apparent connection with any electro-dynamic theory.

What has led me back to the very interesting address delivered by Arrhenius four years ago in the Royal Institution is his book, 'Worlds in the Making,' which has been published this year in London.

In that work Professor Arrhenius gives a description of

the experiment shown in the Royal Institution by Professor Nichols, in May, 1905.

The initial point of the argument used by Professor Nichols seemed to me at the time to be, that, as the force of radiation pressure acting on any body is as the square of the diameter, and the resistance to the pressure as the cube, the smaller of any two bodies will be more easily moved by a given force of pressure than the larger body; but, in fact, in the experiment the bodies which we saw pushed away from the light whilst others mixed with them fell vertically, had thirty times less specific gravity than the latter; and therefore the argument was not rigidly illustrated by the experiment. Notwithstanding that discrepancy the experiment is one of great interest and importance in connection with the action of light, which was, I think, first demonstrated by Crookes's radiometer. It was not clear, however, that the lens which focussed the light of the electric lamp used by Professor Nichols did not make his experiment an illustration of the action of heat rather than of light.

The fact of extraneous pressure against spheres being as the squares, and the resisting force of gravitation as the cubes of their diameters, seemed to me to be given as if it had only recently been recognised. It has, however, long been familiar to me in connection with the action by which grains of sand, or pebbles, are rolled along the bed of a stream. That I believe to have been long ago recognised in some engineering works, and I built upon it when more than thirty years ago I pointed out that a star exceeding the sun in size and splendour, but similar in all other respects, might be so far away as to be out of reach of the strongest of our telescopes, but, nevertheless, be exerting a force of gravitation in the solar system exceeding that of the sun; because the force of gravitation is as the cube of the diameter and the visibility only as the square. For that same reason the pressure of a

stream against round pebbles of different size is as the squares of their respective diameters, and the resistance to the action of the stress is as the cubes of the diameters; so that, therefore, the ratio of pressure to resistance is greatest in the smallest pebbles.

I find Arrhenius rather hazy in his treatment of the sun's radiation pressure; but his description of the changing phases of the corona is quite in accordance with the action of the forces described in a paper which I offered to the Royal Geographical Society in the year 1876. There is nothing hazy about that paper. It is a clear and explicit demonstration of the circulating action in the ocean caused by the earth's rotation which is equally applicable to the effect of the sun's rotation on any fluid, or haze, resting on the sun; and the recent discoveries of the movements of the sun's corona are a splendid corroboration of the theoretical action described in that paper, which covers two octavo pages, or, with a supplementary proposition, three pages.

I return to Dr. Larmor's lecture of 1st May because I strongly object to Newton being claimed by the Etherites as belonging to their school. Dr. Larmor referred us to a paper of his in the 'Chemical News' of 6th March, and in that paper he alludes to Newton as having dealt with the structural connection of atoms with the ether, and specially alludes to an 'atomic theory' given in his 'Opticks'; what I find in the 'Opticks' is Newton's explicit statement that, 'It is manifest that the heavens are void of all sensible resistance and by consequence of all sensible matter'; and that is the view which pervades the 'Principia'; being, in fact, necessitated by the main argument of that work as regards the interaction of centripetal and centrifugal force.

In the paragraph to which Dr. Larmor specially refers I find Newton says: 'Matter is not necessary in all places.' And just before that he says that matter is formed of solid,

massy, hard, impenetrable, moveable particles; and that 'the changes of corporeal things are to be placed only in the various separations, and new associations, and motions of these permanent particles.'

Tell me, Sir William Crookes, are not Arrhenius and Larmor mistaken in supposing that modern science stands with Newton as regards his 'atomic theory' as above stated in his own words? Is not leading opinion now more nearly in conformity with my argument to the effect that in the transformation from water to ice there is a transformation of the constituent atoms, and not merely a 'new association' of them?

I do not say that Dr. Larmor has no basis for connecting Newton's name with ether pressure, for I long ago noticed that when perplexed by charges of being atheistical in his idea of gravitation acting across empty space all the way from the earth to the moon, he tried to create material connection; but when so doing he forgot the principles of his own 'Principia.'

With all due deference to Arrhenius and to Larmor, I must say it seems to me that the attempt to validate Newton's atomic theory has failed as completely as the attempt to invalidate his law of gravitation.

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

To Sir William Crookes, D.Sc., F.R.S., Secretary, the Royal Institution.

25th June, 1908.

Dear Sir,—In a letter which I addressed to you on 11th April, 1906, I commented on an important gyroscopic experiment which Professor Thomson had exhibited in one of

his lectures shortly before that date: I believe in that of 7th April.

I take the annexed two diagrams from page 124 of the 'Essays on the Action of Astral Gravitation' which I published in the year 1900, as they serve to show accurately the character of the experiment placed before us by Professor Thomson.

DIAGRAM II.

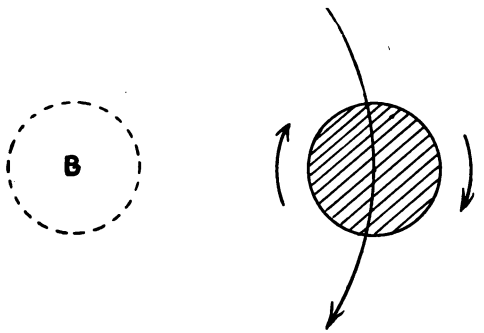


FIG. 1.

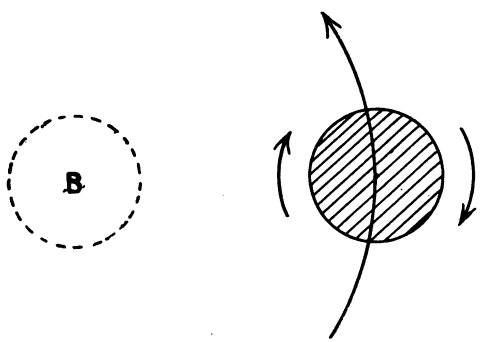


FIG. 2.

A rotating gyroscope suspended by a cord in the position B in figs. 1 and 2 Professor Thomson moved from the vertical, so as to give it a motion of revolution in addition to its motion of rotation.

At one time the revolution was given in the same direc-

tion as the rotation, as in fig. 1; and at another time in the opposite direction to the rotation, as in fig. 2. And he showed us that in one direction the motion of revolution was more rapid than in the other.

I did not succeed in following the argument by which Professor Thomson made the vast velocities of the vibration motion of his infinitesimal corpuscles explain the difference in the velocity of the gyroscope's revolution under the two different conditions shown in figs. 1 and 2. I have, however, shown not only in connection with the diagrams I have reproduced here, but in many other parts of my works, that under the conditions shown in fig. 2 the motion of revolution is less resisted than under the conditions shown in fig. 1; and I presume that to have been the case in Professor Thomson's experiment.

That is so because the resistance of astral gravitation to motion is as the square of velocity.

Let the velocity of revolution on the outside edge of the disc be 8 and on the inside edge 6. And let the rotational velocity of the periphery be 24.

Then in fig. 1 the resistance to the revolution is $(8+24)^2=32^2=1024$ on the outside; and $(6-24)^2=-18^2=-324$ on the inside: together $1024-324=700$. And in fig. 2 it is $(8-24)^2=-16^2=-256$ on the outside; and $(6+24)^2=30^2=900$ on the inside: together $-256+900=644$.

That makes the resistance to the motion of revolution under the conditions of fig. 1 $=700$; and under the conditions of fig. 2 $=644$; thus giving a clear reason for the revolution to be more rapid under the conditions of fig. 2 than under those of fig. 1.

I have in the foregoing taken the velocity of rotation as decidedly greater than that of revolution because that was clearly the case in Professor Thomson's experiment, as it also was in those specially alluded to in connection with my

diagrams. If, however, we reverse the relative velocities, then we have in fig. 1 the resistance to the revolution $(24+8)^2=32^2=1024$ on the outside edge; and $(18-8)^2=10^2=100$ on the inside edge: together $1024+100=1124$. And in fig. 2 it is $(24-8)^2=16^2=256$ on the outside; and $(18+8)^2=26^2=676$ on the inside: together $256+676=1132$. That reverses the result as regards resistance, making it greatest under the conditions of fig. 2.

I refrain from attempting to elaborate this point further at present, because it seems to me that you have now before you Professor Thomson's experiment explained in a more clearly defined manner by the action of astral gravitation than by his corpuscular theory as expounded at the lecture to which I have alluded. *Quousque tandem——?*

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

To Sir William Crookes, D.Sc., F.R.S., Secretary, the Royal Institution.

3rd July, 1908.

Dear Sir,—As in the letters I have addressed to you it has been requisite for me in several instances to point out that the theory of astral gravitation had originated some now prevalent ideas, or discoveries, which were when first announced scouted by all leading scientific societies and journals, I read with keener interest than most men the leading article in yesterday's 'Times' on the Darwin-Wallace celebration.

I cannot suppose that there is any scientific man who does not appreciate the manner in which Darwin and Wallace treated each other in connection with the discovery of 'natural selection' as being a line of conduct which redounds

to the credit of the scientific world. It does not concern the scientific world alone, for it is, in fact, a bright example of the highest instincts of human nature.

But the general tenor of the article is in disparagement of any claimant in disputed questions of priority, which is a matter which did not arise in the Darwin-Wallace episode. Is it to be supposed that if either Darwin or Wallace, or the two jointly, had in the first instance been rejected by the learned societies, they would, after publishing on their own responsibility, have thought it right to stand by in silence whilst the same societies long afterwards accepted their views presented by a plagiary? Might they not perhaps have been more likely to feel it to be a duty to face discomfort, disparagement and abuse, to prevent if possible the triumph of injustice?

I presented my theory of astral gravitation to the Royal Geographical Society without any thought of publishing on my own responsibility until after it had been rejected. Would Darwin, or Wallace, have had so little confidence in their views, or so little appreciation of their value, as to have refrained from publishing on their own responsibility if the learned societies had rejected them?

We are all entitled to admire Darwin's self-control in making his discovery a secret for twenty years, without on that account failing to be in sympathy with the more impetuous course to which Wallace was impelled when his investigations in the forests of the tropics led him to the same discovery. I was neither so reticent as Darwin nor so impetuous as Wallace; thirteen years before offering my paper I had clearly recognised that the action of astral gravitation excludes the so-called First Law of Motion from the realms of practical science; and two years later a loitering voyage on the Parana led me to recognise the action of the earth's rotation on that river in a manner which gave

a clue to its action in producing the general features of the existing circulation of the ocean.

I think that according to the general tenor of the 'Times' article this letter of mine is very reprehensible, and I am sure that the writing of it gives me no pleasure.

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

To Sir William Crookes, D.Sc., F.R.S., Secretary, the Royal Institution.

8th July, 1908.

Dear Sir,—The question is, What are these little corpuscles which you and your colleagues on our committees like better than astral gravitation?

Newton, believing light to be a material substance, wanted them as the atoms of that substance and made them dash with the velocity of nearly 200,000 miles a second from the sun to the earth; or from a tallow candle to the pages of a book read by its light.

But now (*pace* J. J. Thomson) nobody wants the little things to do such a pace as that. Young and Kelvin, following the main idea of Huyghens, have required them merely to vibrate, and become luminiferous as they vibrate, as waves are propagated through them from any source of light; and I understand that the velocity of motion which Kelvin has recently required them to effect in their vibrations is reduced to a rate of 100 or 120 miles an hour.

Young supposed these corpuscles, or atoms of ether, not to be obliged to get out of the way of the earth as it moves through space, but to be so small as to be able to pass through the earth—that is, through the walls of our houses, through the chairs and tables in our rooms, and through our own bodies—like water through a

sieve. I have heard Kelvin speak in accordance with that view, though he has more recently denied the possibility of such action, on the ground of a demonstration to the effect that the ether cannot be a fluid, as had been supposed, but must of necessity be an elastic solid, making it impossible for the atoms of the ether to pass through our bodies, like wind through a tree, without the atoms of the ether and the atoms of our bodies occupying the same space at the same time. That is professedly arrived at by argument remorselessly followed to its logical conclusion, and it might, I think have led most men to suppose any claim of the ether to existence as a material substance to be obliterated by such a conclusion. But with Kelvin the resources of science were not exhausted, and he boldly adopts what he declares to be the only way to protect the ether, by simply denying 'the scholastic axiom that two portions of matter cannot jointly occupy the same space.'

I beg you, Sir William Crookes, rather than let go of that common sense 'scholastic axiom,' to let 'the ether' perish and recognise that so-called ether pressure is merely the opposite aspect of astral gravitation, as argued throughout this one-sided correspondence.

Granting the conclusion arrived at by Kelvin, to the effect that the earth and the ether must occupy the same space at the same time, there is a way of accepting that conclusion without abandoning the old axiom that two atoms of matter cannot occupy the same space at the same time.

Lord Burleigh and his nod were in the same place and also the wisdom of which his nod was significant, because the latter was one of his attributes, and the nod his own resultant movement. And the alternative to denying the old axiom when Kelvin's argument makes the particles of the ether occupy the same space as the particles of the earth

is merely to admit that the argument has reduced the ether to the status of being an attribute of matter.

Matter and inertia are identified, so that the quantity of the former in any given space is the exact measure of the latter in that same space ; and gravitation is the force exerted by each material particle as connecting links extending between it and every other particle of matter. Has not Kelvin's argument in fact reduced the ether to the status of being the force of gravitation exerted at every point in which a particle of matter exists ? He has located it in matter in the sense in which the force exerted by the hand is located in the hand.

A remarkable episode in connection with this question is the passage of an eminent statesman through it like a comet from the outside regions of the realms of science. In Mr. Balfour's suggestion to the effect that modern science not only explains matter, but explains it away, may be vaguely traced a distorted shadow from the doctrine of evanescence which makes material existence a passing phase linking together pre-material and post-material forces.

But if science is really with Mr. Balfour in the idea that ' without the ether an electric theory of matter is impossible,' then the electrical basis of matter described by him is destroyed by Lord Kelvin's latest argument, except among those exceptional philosophers who are able with him to believe that two particles of matter can be in the same space at the same time. Perhaps Lord Kelvin had before him a space of four dimensions ; and, whilst putting one particle into the three dimensions known to our school books, put the other into the fourth dimension, which is nobody knows where.

The conclusion is that the tiny corpuscles which are supposed to constitute ' the ether ' are infinitely more tiny than any etherite supposes ; which logically means that they

have no material existence ; and therefore we need not impose on ourselves the idea that two material particles can jointly occupy the same space, or that space can have more than three dimensions.

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

To Sir William Crookes, D.Sc., F.R.S., Secretary, the Royal Institution.

24th July, 1908.

Dear Sir,—I know that I did not write the leading article in the ' Times ' which originated the letters which I addressed to you on 3rd and 8th instant ; and I am sure you did not write them, because you could not have done so with my correspondence of the last two years before you without truly realising the situation and then feeling it to be unworthy of the high position you hold in the scientific world to make the Royal Institution under your guidance a shelter for misappropriation of renown and the rewards which accrue to its possessors.

I recently alluded to the letters I have addressed to you as constituting a one-sided correspondence ; but they are so only in a technical sense, or from a narrow point of view Broadly seen from a loftier point of view, your part is performed by many lecturers speaking under your secretarial wings with contorted fragments of my work as the unacknowledged basis of their discourse.

I have in some of my preceding letters pointed out that Thomson and Tait, in the first edition of their ' Treatise on Natural Philosophy,' declared that they gave Newton's laws of motion in his own words because they considered it impossible to improve his method of treating the subject. That was published in 1867, the year following my attack

on the so-called First Law of Motion. That treatment of the subject by Thomson and Tait I then regarded as the greatest difficulty I had to contend against, and it caused reiterated attacks on that law from different points of view to be a prominent feature in my publications. I am not now going to repeat arguments; but ask you to consider as to whether Tait's treatment of the 'first law' sixteen years afterwards, in the 'Encyclopædia Britannica,' published in 1883 (and alluded to in my letter of 6th May), is not a practical vindication of my attack on it, and the new position then assumed towards it illogical. Is it not evident that some recognition of my view regarding that law ought then to have been made, or, at least, an admission of the existence of a reasonable basis for my arguments?

After I had expelled the so-called First Law of Motion from my philosophy I was for fully five years perplexed as to the true source of an important part of the mechanical action it was supposed to perform in the solar system. You can in your imagination make the radiation pressure of sunshine sustain the planets in their orbits against the action of the sun's gravitation; but the imagination of the boldest of poets in the finest frenzy rolling could not, I think, make earthshine sustain the moon in its orbit against the earth's gravitation; and we all, I think, admit that the sustaining force in the two instances must be of similar character.

The *push* of radiation pressure must thwart the free action of the so-called First Law in interplanetary space as certainly as the *pull* of astral gravitation; but whilst the latter explains the vagaries of the gyroscope by the same action as that by which it sustains the moon in its orbit above the earth, or the earth in its orbit above the sun, radiation pressure is baffled by even the single experiment shown by Professor Thomson on 7th April, 1906, and dealt with in my letters of 11th *idem* and some subsequent dates.

I challenge you, Sir William Crookes, and all your etheritic colleagues to give any reasonable explanation of the phenomenon exhibited by Professor Thomson without the assistance of the simple action of astral gravitation which I have described.

Radiation pressure as certainly expels the 'First Law' as does astral gravitation, and they both require the transverse action of gravitation to sustain the continuity of motion supposed to be supplied by that law; but the acceptance of the theory of ether pressure could not directly lead to the discovery of that transverse action, which was a natural sequel to the discovery of the action of astral gravitation. The transverse motive force and the astral retarding force are both simple extensions of Newton's law of gravitation.

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

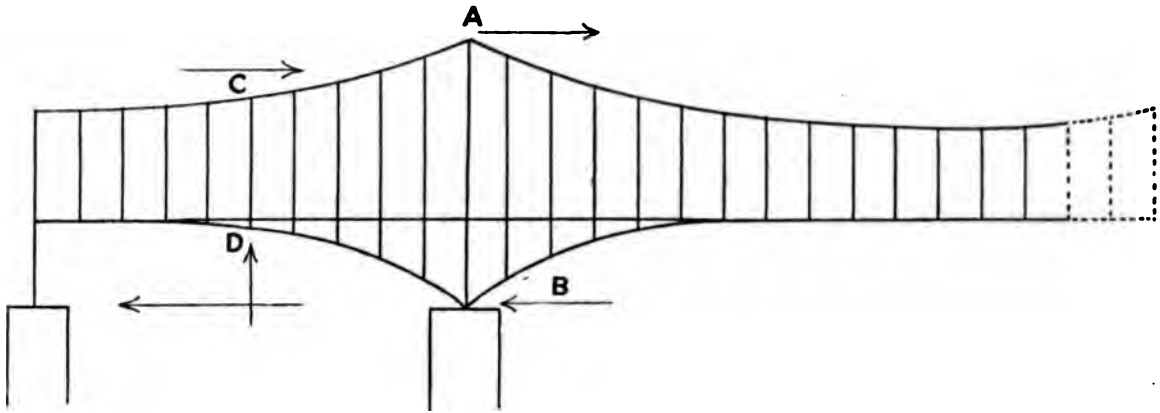
APPENDICES

WHEREVER in the following correspondence the words 'this Institution' occur without being applicable to the Institution of Civil Engineers they must be understood to mean the Royal Institution.

DIAGRAM III.

SOUTH HALF OF THE QUEBEC BRIDGE.

The dotted part had not been completed when the bridge collapsed in the afternoon of 29th August, 1907.



Bending Stress at A B on 29th August, with the foundation of the River Pier as a fulcrum, 8,500 tons.
Stresses at C D on 29th August, as described in connection with Diagram IV., 19,000 tons; the River Pier acting as a vertical fulcrum.

APPENDIX I.

IRON BRIDGES.

To Dr. J. H. T. Tudsbery, Secretary, the Institution of Civil Engineers.

15th April, 1908.

Dear Sir,—In designing the new Tyne Bridge, Dr. Harrison has adopted in principle the only method by which the vibration explained in my letter of 8th March, 1900, can be avoided; and his plan of five girders for carrying four lines of rails is one which I expect to see before long followed in the construction of all such bridges: four girders being given for three lines of rails, and three girders for a double line of rails. There is, however, in Dr. Harrison's design a survival of the old mischief which might easily have been avoided, and might perhaps (as well as I can judge from the small cross section which does not show detail) be remedied without too great an expense.

According to the cross section, the traffic stress is allowed in part to bear directly on the diagonal struts, and it must therefore create lateral vibration (with subsidiary vertical vibrations) of a character which would be avoided by making the traffic stress bear directly on the vertical support given by the girders, leaving the diagonal struts to do only their natural work of keeping the girders from getting skewed from their rectangular arrangement.

I am, dear Sir,
Yours truly,
WM. LEIGHTON JORDAN.

P.S.—*12th May.*—Let A B (fig. 1 in Diagram IV.) be the top part of a girder in cross section; C D a transverse girder on which the permanent way rests; and E a diagonal strut partly supporting the transverse girder.

Then the vibration above described (15th April) is com-

pounded of the horizontal thrust, *v w*, and the vertical thrust, *x y*. And by clamping the cross girder on the main girder, as in fig. 2, the clamp would form an 'anchorage' against bending stress at D.

In the bridge under discussion the extent of the mischief caused by the stresses shown in fig. 1 is the creation of a vibration which must tend gradually to make its joints rickety. Those stresses may in that bridge be considered as embryonic, though identical with what may under other conditions of length of leverage, both horizontal and vertical, become of such paramount importance as to cause instantaneous breakage of the struts on which they act, instead of merely a tendency to loosen their joints.

DIAGRAM IV.

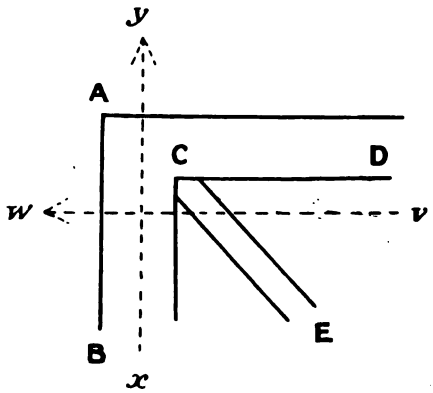


FIG. 1.

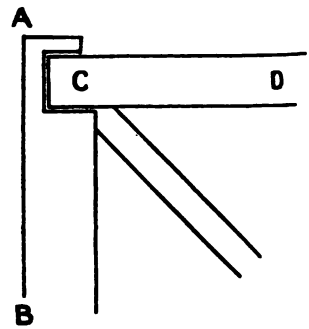


FIG. 2.

It seems to me that those stresses, which in the King Edward VII. Bridge may be termed embryonic, as they are not sufficient to do more than cause vibration, became developed in the Quebec Bridge at the moment of its fall, to a force of about 19,000 tons in the *anchor* arm, where not much more than 8,000 tons is the generally estimated amount of stress (4,000 being tension along the top chord and 4,000 compression along the bottom chord). I have calculated the stresses in the *cantilever* arm at the moment of the fall on the same principle as in the *pro forma* calculation I gave in connection with Professor Fidler's recent paper on cantilever bridges; and (as shown in the table enclosed herewith) it gives 8,546 tons as bending stress on the main posts

of the river pier at the time the bridge collapsed. As half of that is tension along the top chord, it agrees with the generally accepted uplift of about 4,000 tons on the anchorage.

The table shows that the stress which in the position of the main posts would, against one foot leverage of resistance be 2,692,400 tons, becomes against the actual leverage of 315 feet, 8,546 tons. (The table is appended on p. 84.)

If the feet of the main posts were firmly supported (during construction of the cantilever arm) against yielding to the compression thrust of 4,273 tons, then the corresponding tension stress along the top chord would be limited to the 4,273 tons all the way from the main posts to the anchorage. But instead of that being so, the stress which at the main pier was met by a resistance leverage of 315 feet was allowed in the centre of the anchorage arm to become concentrated against a leverage of only 140 feet, giving a stress of 19,230 tons, being 9,615 tons' tension along the top chord and 9,615 tons' compression along the bottom chord; and the latter would to a great extent take the direction of uplift whilst the main posts of the river pier remained unbroken.

The stress in the centre of the anchor arm was thus more than twice as great as in any part of the cantilever arm, and would have continued to be so during construction until the two cantilevers met over the river and gave each other reciprocal support.

WM. LEIGHTON JORDAN.

Extract from the Official Report of the Discussion on Cantilever Bridges on the 11th February, 1908.

This can be followed only with the assistance of the diagram referred to as printed by the Institution of Civil Engineers.

Re PROFESSOR T. C. FIDLER'S PAPER ON CANTILEVER BRIDGES.

Mr. Wm. Leighton Jordan said there were three points in connection with the cantilever paper on which he would like to make a few comments. Although he was not an engineer, it was a matter he had studied a great deal. He did not think the author had driven home two conclusions to which he had clearly pointed in his paper, and in no publication had he found

those points so prominently brought to his notice as when reading Professor Fidler's paper. He mentioned with great trepidation, but with confidence, that Professor Fidler in his formula had taken what was not an appropriate quantity by dealing with l^2. In considering the stresses at any point in a cantilever, when dealing with a single unit it was necessary to take l^2 as the measure of the stresses to be dealt with, but directly an aggregate mass was dealt with l^2 was not a proper measure, as if it was used it was necessary to have a denominator to correct it, and that denominator altered under different circumstances. In the paper where l^2 was dealt with the denominator was taken as 8 for the stress of $\frac{1}{2}\ w\ l$ at A_2B_2; but, for the stress of that same extension at C E, the figure 2 would be correct. If instead of l^2, in dealing with any aggregate mass one took $l \times \frac{1}{2}\ l$, i.e. what was called the sesquialteral, instead of the square, it came to less than the square and required no correction. He could not imagine that the formula given for the temporary stress at A_2B_2 had been used in actually building a cantilever, or any other bridge, and that an error must have got in there through some inadvertence. The dimensions in the diagrams seemed rather different from what they were in the book, but nevertheless he thought he could give the figures. For instance, taking the diagram of the cantilever ending at C E, and supposing it secure up to that point, the question was one of adding on, and in the paper it was dealt with as extended to z, which was a quarter of the independent girder. Taking that, as a simple quantity, as 20 feet extension—it might be anything—and taking it as 1 ton per foot length, the weight to be dealt with was 20 tons. That gave 400 tons for l^2, and it was quite impossible to get that out of the 20 tons as a stress at C E except the whole 20 tons were imagined to be carried out to the extreme end at which one was working, and then there was no material to support it; there was only an imaginary support remaining. The maximum to be obtained with the extension was what he had given, $w \times l \times \frac{1}{2}\ l$, which was the sesquialteral proportion, and that gave only a stress of 200 tons and required no denominator. The difference on applying Professor Fidler's formula to the stress at A_2B_2 seemed to be in the direction of safety in constructing the cantilever, but it was the business of the mathematician to give a correct formula and leave it to the engineer to give the

safety. If the formula was used in the opposite direction there would be an element of danger in it.[1]

There were two other points which he thought had been overlooked, or, at any rate, were not stated. In extending beyond the cantilever there could be no stress in the extension that could do any harm before the bridge gave way at B_2. The whole thing would come down at B_2, because the main stresses came in there. Following the diagram, beginning at A_0B_0, the ratio which the vertical bore to the arm extending from A_0 outwards to C was greater than it was further on. The vertical decreased in proportion to the horizontal arm until B_2 was reached, where the minimum was obtained, and passing from B_2 the maximum was reached again and went on increasing, i.e. the ratio between the vertical and the extension beyond the vertical, and therefore at the point of the minimum ratio of the vertical to the horizontal there was a weak spot, and that was the point of weakness at which it must come down.

A third point he wished to mention was that it did not seem quite clear that all the material was required whilst in construction. Most of the material of the middle part was not required until the bridge was completed, and therefore the material could be passed out partially in narrow pieces. If in any cantilever B_2 was strong enough for the stresses that were to be brought upon it when in working order, it was quite strong enough to carry the middle part right across until the two met, not by building it all out perfectly, but by carrying it out in threads and meeting there.

[1] Let l = 80 feet
A_2C = 30 „
A_2B_2 = 12 „
CE = 8 „
and w = 1 ton per foot length 8 feet deep.
Then $\frac{1}{2}\,l + A_2C = 70$ feet; and $w \times l \times \frac{1}{2}l =$
$1 \times 70 \times 35 = 2{,}450$ tons
deduct 450 tons ($=1 \times 30 \times 15$) for part of stress of A_2C
gives 2,000 tons stress against leverage of C E; or (for ratio of leverage A_2B_2 to CE as 1·5 : 1) $= \frac{4000}{3} = 1{,}333\frac{1}{3}$ tons for stress of $\frac{1}{2}\,wl$ at A_2B_2, representing its bending moment on that vertical if unsupported at F G.

Professor Fidler's formulæ for that bending moment are $\frac{1}{2}\,wl\,(X_2 + \frac{l}{4})$, which I make = 2,400 tons; and a second formula is given as $M_2 + \frac{wl^2}{8}$, which I make = 855 tons; the one giving a large margin of safety and the other a smaller margin of danger, unless I do not properly follow Professor Fidler's method.

APPENDIX II.

SCOUR IN TIDAL RIVERS.

Extract from the Official Report of the Discussion on the Netravati Bridge on the 24th March, 1908.

Mr. Wm. Leighton Jordan remarked that an important point appeared to him to be clearly illustrated in the diagram of the Netravati Bridge, and that was with regard to the action of the scour. The thin black line showed the former position of the sand, and the blank space below it what had been scoured since the bridge was built. Of course much more had been scoured away than had been filled in above that line, simply because the river must make a deeper passage for itself in consequence of being narrowed. But on looking at the diagram it was very clear that there were two channels, one near the south side and the other near the north side, and between them the sand had been piled higher instead of being washed away. That, he contended, was absolutely necessary wherever there was a tidal channel of that sort. The tide as it came in under the bridge would not go out by the same channel; there might be very little difference, but a difference there must of necessity be, because as the water was moving the rotation of the earth must affect it, even if slightly. The diagram clearly indicated what for years past had been said not only by himself but by others as occurring, and he contended that no engineer would ever manage a channel of that sort without clearly understanding that action. The tide would not go out by the same channel if it could possibly find another. It could be got closer together by narrowing that channel, and he believed quite successfully. . . . If it were possible for a tide to scour away on each side as it did in the open sea, it would gradually widen apart, make a broader bank, and sooner or later the channel would change. In the diagram it was prevented from changing, and that no doubt would be kept up as long as the bridge was kept standing.

To Dr. J. H. T. Tudsbery, Secretary, the Institution of Civil Engineers.

29th May, 1908.

Dear Sir,—In the proof which I received from you on 14th instant and returned on the same day I notice that an explanatory statement based on what I said in the discussion of the sand banks of the Hooghly River, about three years ago, had been suppressed before sending me the proof. I now write to ask you kindly to consider the expediency of allowing my reference to the Hooghly River to appear in your Report of the discussion. And I further ask that, instead of omitting that reference, the argument I gave in the Hooghly River discussion may be given as a footnote attached to what I said on the Netravati River with a statement to the effect that it had been omitted from the report of the Hooghly River discussion through a misunderstanding.

I am, dear Sir,
Yours truly,
WM. LEIGHTON JORDAN.

The contribution to the Hooghly River discussion referred to in the foregoing letter is given in the following extract from the official report of that discussion, except that from the words ' James and Mary,' in line 30, to the words ' and when,' in line 36, was accidentally omitted in the office of the Institution.

Mr. Wm. Leighton Jordan said he wished to draw attention to some facts connected with the shallows in the James and Mary Reach and in the Royapur and Moyapur Reaches, higher up. The momentum of the flood tide passing the sharp bend at Hooghly Point carried it past the descending stream, so that it commenced the ascent of the James and Mary on the right bank of the river; having, therefore, in relation to the down stream, an anti-cyclonic position, and there was confusion of currents, which caused a three-fathom shallow. Above Fulta Point the flood tide reached the left bank of the river and kept to that bank up to Brul Point, having, therefore, all along that reach a cyclonic position, and when the flood tide was on that side there was nothing less than five fathoms; though where the circulation was anti-cyclonic there were three fathoms. At

Brul the flood tide crossed to the other side, and immediately above there was a shallow of three fathoms. Higher up, just before the flood tide went over to the right bank of the river, there were three fathoms again. He had pointed out, in connection with the Dublin Harbour, the manner in which any influx of water into the harbour, not brought in with the momentum in the direction of a cyclone, tended to create confusion in the harbour and to cause sediment, instead of running smoothly with the circulation. If brought in in the form of a cyclone, so as to take cyclonic circulation, it would run smoothly and get out. These reaches were an example of this. Where the flood tide was in a cyclonic course there were five fathoms; where it was not so, but was forced over to the opposite side of the river, there were shallows. He considered that wherever a tide branched off from the parent tide there was a normal tendency to cyclonic circulation. He believed the main force of the rising tide came up the Hooghly estuary to Sangor Island by the eastern channel, giving an indication of a cyclone in that part. In the Bay of Bengal itself the tide was an anti-cyclone; it was a tide directly raised by the sun and moon; but in such an offset from it as the river the tide tended to take the form of a cyclone. It had nothing to do with the gravitation of the sun or moon, but was dependent on the earth's rotation. As long as it could keep its cyclonic circulation it ran smoothly, but when once that was broken by any cause whatever there was confusion and sediment. The action of the earth's rotation on the water in the river would never be correctly understood until it was recognised that, wherever the moon raised a tide by its direct action, that tide was an anti-cyclone, exactly as in the Bay of Bengal. In the Southern Indian Ocean there was another anti-cyclone, and also another in the South Pacific and in the South Atlantic. The southern edge of those anti-cyclones was in the great Southern Ocean. They ran one into the other just round that part of the world where it was generally stated in all text-books on the tides that the tide ran from east to west, and the derivative tides came from that. That view was first put forward by Lord Bacon, and his authority had dominated everybody, and there had been a general disinclination to dispute it. It had been disputed from time to time, but little attention had been given to it. Until it was recognised it would never be possible properly

to connect the action of the earth with river motion. As a matter of fact, the tides came from the Pacific to the Atlantic round the Horn from west to east. At the Cape of Good Hope, round the south of Africa, the tides came from west to east, and all round the south of Australia the tides were also west to east. Where the tide was supposed to run from east to west, it really ran in the opposite direction, and no such tide existed as that which was supposed to be the predominant tide in the world, of which all the others were offsets.

To Dr. J. H. T. Tudsbery, Secretary, the Institution of Civil Engineers.

16th April, 1906.

Dear Sir,—As a sequence to my letter of 10th instant, which was called for by the letter received from you that day, it has clearly become my duty to confirm my letter of 2nd December last and those preceding letters on which it was based.

The position is that—

1. On the 17th January, 1905, Mr. Vernon-Harcourt read a paper in which he specified the most important of the sand-banks which obstruct the navigation of the Hooghly River, and proposed works for guiding the tidal action in the river for the purpose of their removal.

2. I gave in the discussion an explanation applicable to each one of the specified sand-banks, showing how they are created and maintained by the tidal action.

3. Your 'Publication Committee' could not possibly have had my argument properly before them when they considered it not to be applicable to the subject of the paper.

4. Suppose Professor Vernon-Harcourt had been so chivalrous as to insist on replying to my argument; you could not then have suppressed it. Professor Vernon-Harcourt and I have for more than thirty years held rival views regarding tidal action, and his acceptance of the protection of your 'Publication Committee' to enable him to escape the responsibility of replying to my argument, spoken in his presence, is a course on which I will not comment.

The character of the Institution, as regards honesty of purpose in its discussions, combined with precedent study of the subject discussed more readily gaining consideration than personal

favouritisms, is in the hands of the Council rather than in those of individual members. . . .

5. Sir Guilford Molesworth was President of the Institution at the time of the discussion, and himself took part in it. It is mere matter of fact that my remarks were directed to the main point of Professor Vernon-Harcourt's paper; whereas it is no disparagement of Sir Guilford Molesworth's very interesting remarks to say that they did not grapple with the difficulties, the removal of which was Professor Vernon-Harcourt's main object.

In spite of that Sir Guilford Molesworth acquiesces in the action of your 'Publication Committee' in reporting his remarks whilst persisting in the suppression of my argument on the false pretence of its not being applicable to the subject of the paper.

* * * * * * *

It is a comparatively small matter for erroneous opinion sometimes to find place in our published Proceedings. Diametrically opposite views are given in discussions where one must of necessity be absolutely wrong; but the essential point for the maintenance of the character of the Institution is honesty of purpose in discussion and the maintenance of fair play by the Council between rival arguments which are known to be results of precedent study of any subject formally allowed by the Counci to be brought forward for discussion.

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

To Dr. J. H. T. Tudsbery, Secretary, the Institution of Civil Engineers.

4th June, 1903.

Dear Sir,—I think it would be quite out of place for me to refrain from pointing out, in reply to your letter of yesterday's date, that the 'misunderstanding' referred to in my letter of 29th ultimo is recorded in your letter of 4th July, 1905, where you tell me that our 'Publication Committee' had decided that what I said when I spoke in the Hooghly River discussion was not applicable to the subject matter of the paper.

It became clear at the time that the Committee were misled through not having a correct report of what I said before them;

because, in fact, the main point of the paper was directed to the obstructions caused by sand-banks in the river, and I gave an explanation of the manner in which the conflicting action of the tides and the river current tends to cause such banks, and the proper relative positions to give (where possible) to the tidal and the river current in order to keep a deep channel open—that being a subject to which I had given special consideration from time to time during the previous fifty years.[1]

The Committee are entitled to consider my cyclonic action of the currents wrong, and Mr. Ellis's straight course action correct; but to say that one and not the other is applicable to the subject-matter of the paper is simply at variance with the clear facts of the case.

I therefore again urge the expediency of the course suggested in my letter of 29th ultimo being followed.

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

To Dr. J. H. T. Tudsbery, Secretary, the Institution of Civil Engineers.

8th June, 1908.

Dear Sir,—Your letter of 4th July, 1905, came upon me like a bombshell, as I had not for a moment imagined such an unceremonious exclusion of an argument offered in discussion to be possible in our Institution; and as my letter of 4th instant replying to yours of the previous day stands unacknowledged, I think it expedient to supplement that letter with a further remonstrance against the course which your letter of 3rd instant shows to be contemplated rather than wait until the exclusion of what I said in the Netravati discussion becomes an accomplished fact.

If the principle involved in the idea that knowledge gained in the study of sand-banks in the Hooghly River is not to be referred to in discussing the sand-banks of the Netravati had always been acted on, a large proportion of the most useful discussions in our Proceedings would have been excluded; and

[1] Extracts from my contributions to the 'Buenos Ayres Standard' are given at the end of this Appendix.

in the present case there is a special reason for allowing the action of the currents in the Hooghly to be referred to in discussing those of the Netravati, inasmuch as what I said in the Hooghly discussion was wrongly omitted, and the Netravati discussion chances to offer a suitable opportunity to remedy that mistake. The Hooghly and the Netravati are both tidal rivers in the positions under discussion, and the phenomena in each appropriately serve for illustrating those in the other.

You properly inserted in the Hooghly discussion Mr. Ellis's argument as to the momentum of the river current tending to make it persist in a straight course; but I protest against the exclusion of my argument to the effect that the earth's rotation changes that straight course to a curve in relation to the surface of the earth over which the water moves; and that where a rising tide meets a river current the streams can run smoothly only when able to take such relative positions as to form complementary parts of a cyclone.

That argument would have been imperfect in the Hooghly discussion without showing its connection with tidal action in the open sea; where, as I stated in that discussion, the distribution of cyclones and anti-cyclones is such that all round the temperate zone of the Great Southern Ocean the tide waves flow from west to east, thus constantly *meeting* instead of *following* the moon all round the world in that latitude.

The Proceedings of our scientific societies and the pages of our leading scientific journals have in recent years teemed with elaborate mathematical demonstrations of the manner in which tidal friction westwards must retard the earth's rotation; which is all very splendid as a display of mathematical skill, but is all outside the region of practical science, as in fact the tides give just as much friction eastwards as westwards.

As you tell me in your letter of 3rd instant that you are guided by the Institution's interest as to publishing remarks offered, I ask you to consider as to whether it may not redound more to the credit of the Institution to take the lead in allowing fair discussion to be commanded for the theory I have brought to bear on the sand-banks of the Hooghly and the Netravati, rather than to persist in holding the position of an item among the scientific societies in which a display of mathematical skill is more highly appreciated than commonsense unravelling of

the action of the forces by which natural phenomena are practically controlled.

The gradual growth of the Admiralty tide tables has swept away the waves which Bacon and Whewell supposed to flow from east to west round the Great Southern Ocean; and I, as one who cares as much for the 'general interest of the Institution' as any man among them, ask our Council to take the lead in recognising that practical advance in our knowledge of the world.

A correct knowledge of the scouring action of currents in tidal rivers is of importance to engineers who have to deal with such localities.

I am, dear Sir,
Yours truly,
WM. LEIGHTON JORDAN.

To Dr. J. H. T. Tudsbery, Secretary, the Institution of Civil Engineers.

17th June, 1908.

Dear Sir,—Your letter of yesterday's date makes it clear that a persistence in the exclusion of my arguments regarding the Hooghly River is contemplated, though apparently not yet resolved; and I therefore endeavour to reinforce my letters of 4th and 8th instant in protest against that course.

As you now tell me that there is no question of my desire to promote the interests of the Institution, I would not say it is unfortunate that you allowed a pretension to the contrary to appear to be made, if it were not that you now tell me it is not possible for everything contributed to the Institution to be published, thereby ignoring the special reasons I have given showing that the highest interests of the Institution demand a rectification of the official report of the Hooghly discussion.

I urge upon you that in the actual discussion of the Hooghly sand-banks I explained the curving of the river stream and tidal current in opposite directions under the action of the earth's rotation, and also the circumstances under which that conflicting action tends to build up sand-banks. But, instead of that argument appearing in the official report, there is an elaborate argument based on the idea of the momentum of the currents acting in straight lines without the slightest recognition of those straight lines being affected in relation to the surface of the earth by the movement of that surface.

The official report of the discussion is therefore an absolute misrepresentation of the views held by our Institution as represented in the discussion; and as the Netravati discussion offers a suitable opportunity for rectifying the omission of my argument in the Hooghly discussion, I urge the Council to allow it to appear in the form suggested in my letter of 29th May.

You tell me in your letter of the 3rd instant that as regards publishing remarks offered in discussion the Council are guided by the question of the advantage and general desirability in the Institution's interest. That certainly is what ought to guide them; but they are just as certainly mistaken in supposing it to be to the Institution's interest to suppress views originated by any associate or member who may have given more consideration to a subject brought forward for discussion than any man among them.

What the interest of the Institution calls for is a faithful report of what may be said by the Institution's own experts on any subject submitted for discussion. As regards the question raised in the Hooghly discussion I was the leading expert; and it has become clear to me that some practical engineers are now guided by my views, spoken in that discussion, notwithstanding the Council's refusal to allow them to appear openly in the Proceedings of the Institution. Will that course redound to the highest interests of the Institution?

Secondly, there are several other cases in which the treatment of contributions I have offered on subjects under discussion has been as strange as in the foregoing.

You have now under consideration my contribution bearing on the Quebec Bridge disaster, in connection with which I beg to recall attention to the fact that on 8th March, 1900, I sent you an explanation of the loosening of rivets in the underside of iron girders, which a member of the Council had at a previous discussion declared to stand unexplained. No notice was taken in the Proceedings of my contribution, but the form of girder then in use was soon afterwards modified in various manners, all more or less partially escaping from the mischievous action I had indicated; the last important girder bridge (the King Edward VII. Bridge over the Tyne) being the best yet described at the Institution meetings, though even that leaves great room for improvement, as I have pointed out in the correspondence dis-

cussion on it. I have in that contribution pointed out that the stress overlooked in the construction of the King Edward VII. Bridge was also overlooked in the construction of the Quebec Bridge; but in the latter it had accumulated at the moment of the disaster to such an extent as to create a stress of about 19,000 tons where less than 9,000 was supposed to exist.

I now urge that if my contribution to the subject in the year 1900 had been admitted into the official report of our Proceedings, the attention drawn to the point I had raised, instead of only influencing a few who vaguely heard of it without fully understanding it, would have been properly considered in the engineering profession and would most certainly have prevented any such disaster as that of the Quebec Bridge.

That it may not be thought that I hastily at random inter vened in the matter of bridge construction it seems expedient for me to say that twenty-five years ago I looked forward to being able not only to select the spot for crossing the Uruguay River for connecting Rio de Janeiro with the River Plate by rail, but also to design the general features of the bridge; so the subject was not new to me when I intervened in the year 1900 in the discussion in our Institution.

The cruel death of each of the men who lost their lives in the fall of the Quebec Bridge is a consequence of the want of judgment shown by our Council in suppressing the explanations given in my letter of 8th March, 1900.

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

To Dr. J. H. T. Tudsbery, Secretary, the Institution of Civil Engineers.

1st July, 1908.

Dear Sir,—I am quite in the dark as to whether my letter of 17th June is having consideration; but think it well further to urge that not merely the most ordinary requirements of fair play to me personally, but also the true interests of the Institution make it expedient for the Council to allow my argument regarding the sand-banks of the Hooghly to be reported in our Proceedings, as suggested in my letter of 29th May last.

In connection with the above question I wish now to recall your attention to the discussion of the paper on the steam turbine, read on 5th December, 1905.

In that discussion Mr. Henry Davey asked for an explanation of the backward movement of a loose bush which surrounded the spindle of the turbine. That backward motion of the bush *against* the direct action of the force by which it is set in motion is a phenomenon of the same character as the eastward motion of the tide waves of the temperate zones *against* the course of the moon, by whose force of gravitation the tide is raised. The explanation asked for by Mr. Davey was familiar to me in connection with other phenomena; and as I did not hear it given in the discussion, I sent it to you on 16th December, 1905, with some further details on 26th *idem*. The two explanations I heard at the meeting appeared to me to be at variance with each other, and neither of them conscious of the mathematical evidence given by the figures which I sent to you. Your report of Mr. Parsons's reply shows him to have correctly understood the phenomenon, though I did not myself gather that as I listened to what he said in reply. If I had done so, I would not have written to you on 16th December, 1905.

I think it, however, well now (in face of other omissions of my contributions) to say I do not believe that the clear and explicit explanation I sent to you in writing on the 16th December, 1905,[1] and *a fortiori*, the detail I sent to you on 26th December, 1905,[1] ever existed in writing before those dates; and therefore I think it would have been creditable to the Proceedings of the Institution to be the first to give a printed statement of the mathematical argument as I sent it to you.

Other phenomena of the same character are explained in my published works, but not, as far as I know, in any other published works. The Council can give public credit to colleagues who give them second-hand knowledge, first given in my works, but it would redound more to the credit of the Institution in the long run to give credit to the first discoverer of the general principles by which phenomena of that character are controlled.

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

[1] Now appended to this letter on pp. 43 and 44.

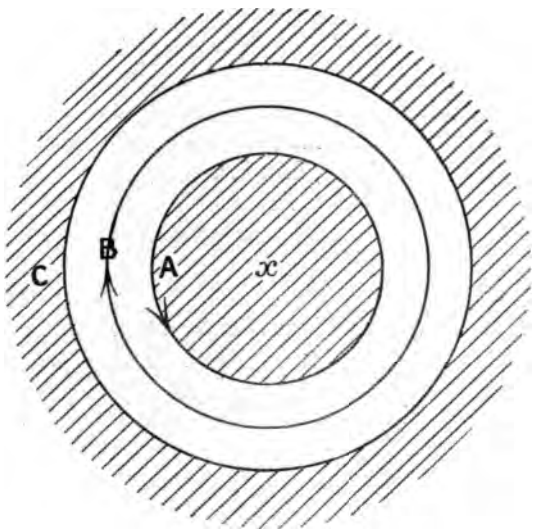
To Dr. J. H. T. Tudsbery, Secretary, the Institution of Civil Engineers.

16th December, 1905.

Dear Sir,—In Tuesday evening's discussion opposing explanations were given regarding the motion of a loose bush, B, placed between a rotating shaft, x A, and a fixed case, C, illustrated in the diagram below.

It was stated that, as a matter of fact, with the shaft rotating 5,000 times a minute, the bush revolved thirty times a minute in the opposite direction.

DIAGRAM V.



A, x. The turbine shaft.
B. Loose cylinder, closely fitting between the turbine shaft and the fixed cylinder, C.

I say that as the bush cannot completely escape the friction caused by the rotating shaft, and as it is free to revolve in either direction round the shaft, it takes that direction and velocity which enable it to escape as much as possible of the friction. I have elsewhere given figures which show how the backward motion of the bush reduces the friction to which it would be subjected if it revolved with the rotating shaft.

The point is that, supposing the bush to give an increase of

1 per cent. to the radius, then, if it revolved completely with the shaft and its friction be represented by $5{,}000 \times 101^2$ (=51,005,000); then the substitution of thirty revolutions backwards changes the relative amount of friction on the bush to $(5030 \times 100^2) + (30 \times 101^2) = 50{,}606{,}030$; which constitutes a reduction of $\frac{1}{4}$ per cent.; in addition to which, I presume, the play of the bush causes a further decrease.

The relative amounts of friction on the shaft, without the bush and with it, would be as $5{,}000^2$ is to $5{,}030^2$ respectively, the backward motion causing that extra friction, unless (as above supposed) the play of the bush reduces the friction which would exist between the shaft and the rigid bearing in contact subject to the same clearance.

In the diagram which I have reproduced I have omitted any special illustration of the 'clearance,' as, in my opinion, merely modifying the friction, and not intrinsically connected with the question at issue regarding the phenomenon of backward motion.

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

To Dr. J. H. T. Tudsbery, Secretary, the Institution of Civil Engineers.

26th December, 1905.

Dear Sir,—If still in time for the discussion of Mr. Parsons's paper, I should like to be allowed to add, in connection with what I wrote on 16th instant, an explanation of the reason why I presume that the play of the bush there referred to tends to reduce friction to a greater extent than the amount there specified.

I doubt that anyone now seriously attempts to question the fact that friction and gravitation are identical forces: the former term being applied to the action of gravitation between bodies in contact or apparent contact.

Let the difference between apparent contact and absolute contact be any amount, say 1, making that figure represent the minimum distance between the shaft, x A, and the bush, B, (diagram of 16th December); and let the mean distance be 2. Then a minimum of 1 at any point creates a maximum of 3 at the opposite point of the shaft. And the retarding action

(whether termed gravitation or friction) being inversely as the square of the distance, is, at the minimum and maximum distances, as 9 is to 1 respectively; giving a mean resistance of 5. To get the position of minimum friction the bush therefore tends to keep itself equidistant from the shaft at all points, as the resistance at each point is then represented by 4 instead of the above mean of 5.

The resistance is as the square of velocity under similar conditions; but the velocity which is sufficient to keep the bush firmly at its mean distance all round changes the condition to one of lower normal resistance; because a lower coefficient of friction is created, which more than counterbalances the normal increase at the rate of the square of the velocity. That reduction of the coefficient of friction with increase of the shaft's velocity of rotation may be indefinitely extended by molecular action on the same principle as that described in connection with the bush. Even when no bush is purposely inserted, centrifugal action must tend to create one and to leave a vacuum between it and the shaft.

I am, dear Sir,
Yours truly,
WM. LEIGHTON JORDAN.

Extracts from the Buenos Ayres 'Standard' of 20th and 30th May, 1871, regarding the effect of the earth's rotation on the river Parana:—

20th May, 1871.—The Parana from its junction with the Paraguay, where it chafes against the rocks of Corrientes and refrains from bearing against and washing away the soft soil which forms the low ground on its right bank, shows a general tendency to bear against the comparatively high and firm ground on its left bank. The rocky shore on the left bank of the Plate also prevents it from swaying away on that side, and it shows no tendency to wear away the comparatively soft soil on its right bank.

This apparent tendency of these rivers to bear against their left banks is corroborated by the streams of the delta of the Parana. The Carapachay, once an open stream, is now choked up with weeds. The stream of the Antiquera is too sluggish to prevent the growth of weeds in it, and it seems likely to be

before long in the same state as the Carapachay. The Capitan is gradually working a way for itself though the Tres Bocas, and if not kept to its present course by artificial means, will cease to run through the Abra Nueva. These all indicate a tendency of the river to bear away against its left bank.

Two causes may be contributing to this result—the one a gradual upheaval of the Andes, together with the land stretching westwards to the river; the other an effort of the river to follow the natural curve which, as it runs from the direction of the equator, the rotation of the earth tends to give it. . . . The tendency of the river shows that works undertaken with the object of throwing the river over towards its left bank, so as to make the stream deep and compact on that side, would be assisting it in its natural course.

If then an attempt be made to make the straggling streams of the Parana and Uruguay run in one deep compact stream to the ocean, it should be commenced by turning all the right bank streams of the delta of the former over towards the Guazu. This might be accomplished by running a railway viaduct like that of the Boca Railway from above the Tigre to the Guazu, and then gradually closing the spaces between the piles without endeavouring too suddenly to force the stream to change its course. Any breakwaters constructed for improving the harbour of Ensenada would have a favourable tendency, and might afterwards be extended for the express purpose of improving the main channel of the river. The harbour of Ensenada can be more immediately utilised than any other point on the river. . . .

30*th May*, 1871.—Delta says that many points of the Parana confute the assertion that it is thrown against the comparatively high land of its left bank by the upheaval of the land on its right bank, and by the earth's rotation. I am aware that in some places the river is at present abrading its right bank; but as long as the general tendency is to follow the course of the high land on its left bank, this occasional abrasion of the right bank is just what might be expected to result from the upheaval suggested. For instance, suppose in a part of the river where the east shore is rock the land be tilted upwards from the west, then the river, not being able to fall away eastwards, must cut away the uplifted soil of its west bank until it makes room for itself at its former level.

Irregularities in the upheaval might also in places cause it to sway at times against its west bank without any insurmountable obstruction appearing immediately opposite. The abrasion now going on between Rosario and San Lorenzo appears to me, judging from the inclination and general appearance of the ravines cut by the rain-water as it runs off the land, to have commenced somewhat recently. Its immediate cause may perhaps be traced to the accidental formation of some obstruction in the river; but I think it more likely that it results from an irregularity in the upheaval of the land. If the requisite information can be obtained, it may perhaps be found that the date of the commencement of this abrasion immediately follows the occurrence of an earthquake in Chili. In the estuary of the Plate the almost complete absence of abrasion on the right bank seems to indicate that, even before it reaches so high a latitude, the rotation of the earth tends to carry the river due eastwards, in the same manner as in the temperate zones it tends to carry the water of the ocean eastwards. . . . The Guazu is the natural channel through which the river endeavours to cut its way; and surplus water pours through other channels only where compelled to take them in order more readily to find its level. If the natural channel be obstructed the river will be constantly endeavouring to regain it by undermining the obstruction; whereas in other channels obstructions once formed tend to become more and more consolidated.

APPENDIX III.

PRESSURE AND STRESS IN MASONRY DAMS.

To Dr. J. H. T. Tudsbery, Secretary, the Institution of Civil Engineers.

30th January, 1908.

Dear Sir,—I have just re-read your letter of 23rd instant, and address you for the purpose of pointing out that in Tuesday evening's discussion no speaker rose to address the meeting without being first called on by name by the President, and I have decided not to make myself exceptional in that respect. If it were the custom for members to rise without being called on by the chair, as in the House of Commons, there would be nothing invidious in doing so, but it seems to me most unfair for the Council to impose such a course specially on me.

I therefore now submit in writing the following new view of the distribution of stress created by water pressure in masonry dams, and also the correction of an important error on that point in the paper presented by Sir John Otley and Mr. Brightmore.

In that paper it is argued that if the width of any dam increases as the depth, then the shearing stress on a horizontal plane must decrease as the height of the plane increases.

That view is arrived at by an error as to the amounts of water pressure at the surface and bottom of the dam, which will, I think, be quickly recognised by the authors if they reconsider the argument they have given on page 8 of their paper.

Eliminating that error, the water pressure is made to vary equably with the width of the dam in question, instead of relatively increasing with increase of depth.

Having cleared that mistake out of the way, I will now point out that what the dam-builder has to face is *the stress created by the pressure,* which stress is in fact precisely the same in quantity,

though not in direction, at all depths; and 'the mean intensity of the shearing stress on a horizontal plane,' dealt with in the paper, must therefore (in that dam) *increase* instead of decreasing as the height of the plane above the base increases.

The fact is that the water pressure increases as the depth; but the surface water acts against the side of the dam with a leverage increased in proportion to the depth; and therefore any increase of the depth of the water increases the thrust, or stress, *at the surface and at all depths*, exactly in proportion as the depth of the dam is increased. Therefore, in a dam retaining the same depth of water all along it, the stress created by the water pressure is the same in amount at all points; but on the inner face there is a partial change from shearing, to tension stress as the height from the base increases; and on the outer face there are increases of compression and shearing stresses corresponding as to quantity with the tension stresses on the inner face.

Sir Benjamin Baker with the jelly on the 28th March, 1905, reminded me of Christopher Columbus with the egg; and, immediately after the meeting of that date, I wrote to ask him if he was aware that the stresses he had described were identical with those by which any pebble is rolled along the bottom of a stream.

In face of the stresses shown by Sir Benjamin Baker's jelly and confirmed by the two independent sets of practical experiments now before the Institution, it is clear that any pebble rolled along by a stream of water gives the clue to the stresses created by water pressure in a masonry dam.

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

To Dr. J. H. T. Tudsbery, Secretary, the Institution of Civil Engineers.

8th and 12th March, 1908.

Table of water pressure and leverage stress against 1 *foot width of the vertical height of masonry dams in each* $\frac{1}{10}$ *part of their depth.*

With unit of depth =1 foot the unit of pressure is $31\frac{1}{4}$ lb.; and for higher dams the unit of pressure increases as the square of the unit of depth.

Depth.	Pressure.	Stress.
1	1	1 × 19 = 19
2	3	3 × 17 = 51
3	5	5 × 15 = 75
4	7	7 × 13 = 91
5	9	9 × 11 = 99
6	11	11 × 9 = 99
7	13	13 × 7 = 91
8	15	15 × 5 = 75
9	17	17 × 3 = 51
10	19	19 × 1 = 19
—	—	—
Total units . 10	100	670

Dear Sir,—Can you kindly arrange for me to substitute the above for a paragraph in my contribution to the correspondence on masonry dams ?

I am, dear Sir,
Yours truly,
WM. LEIGHTON JORDAN.

Addition to table (dated 8th March, 1908) of water pressure and leverage stress in masonry dams.

20th May, 1908.

For the purpose of giving an illustration of the method of using the above table I apply it to the Assuan dam as it is at present, and also as it will be when the proposed increase of its height has been effected, taking, as a basis for the two estimates, Sir William Willcock's recent statement that what is proposed is, to change the head of water of 22 metres to 28 metres.

That constitutes a change from a present height of about 72 feet to about 92 feet.

For the present dam the pressure :—

100 in the table, $\times 7.2^2 \times 31\frac{1}{4} = 162{,}000$ lb.

And the stress :—

670 in the table, $\times \frac{2}{3}$ of $7.2^3 \times 31\frac{1}{4} = 5{,}209{,}920$ lb.

For the proposed dam the pressure :—

100 in the table, $\times 9.2^2 \times 31\frac{1}{4} = 264{,}500$ lb.

And the stress :—

670 in the table, $\times \frac{2}{3}$ of $9.2^3 \times 31\frac{1}{4} = 10{,}869{,}186$ lb.

The pressure being as the square of the depth, and the stress involving the multiplication of the pressure by the depth, the latter would be as the cube of the depth but for the reversed direction in which the figures have to be applied as increasing multipliers.

A point in this method which seems to me still an open question is as to the ratio of the stress unit to the pressure unit which I have taken as a ratio of equality, because it certainly cannot be *less* and I do not see how it can be more.

These estimates of stress are in excess of what has been generally supposed; but in venturing to submit them to the Institution of Civil Engineers I am encouraged by Sir Benjamin Baker's statement at the meeting of 28th March, 1905, to the effect that many mathematicians had been working at the problem of stresses in masonry dams during the previous twelve years, and in that time perhaps half a dozen had been washed away; but he could not mark any great advance in the theory.

WM. LEIGHTON JORDAN.

APPENDIX IV.

FLYING.

To Dr. J. H. T. Tudsbery, Secretary, the Institution of Civil Engineers.

18th April, 1907.

My dear Sir,—Sir Benjamin Baker, presiding at the meeting of the Society of Arts last evening, said that Institution is the only one open to all subjects of general interest.

I do not dispute that statement; but it is of no use addressing it to me. The difficulty with my views has become personal, and my reasons for appealing specially to the Council of our Institution I have clearly stated.

In reference to the action of some small screw-propeller flyers which Major Baden-Powell declared to be 'inexplicable,' Sir Benjamin said Lord Rayleigh considered his mathematics unable to grapple with the question.

In that Lord Rayleigh is completely mistaken: it is not his mathematics that are at fault: the fault is that he does not know how to present the question to his mathematics for solution.

If a carpenter presents his material to the wrong end of his chisel, or to one side of it, he gives the chisel no chance.

Major Baden-Powell's difficulty was that his flyer, instead of progressing in the direction the screw endeavoured to drive it, either plunged head first downwards or, if prevented from doing that, then would not advance at all, but turned over backwards and fell in that manner.

The mechanical action by which *gravitation* turns those flyers over, as shown in Major Baden-Powell's experiments, is clearly and explicitly given in the Report of a lecture which I delivered in Willis's Rooms on the 3rd of November, 1877, which you have in our library; and if Lord Rayleigh would take the trouble to understand the question he would find his chisel quite competent to deal with it.

The result would be either to give an angle at which the screw would be adjusted to the action of gravitation, or else to show it to be impossible for those forces to run together harnessed to Major Baden-Powell's flyers.

In the latter case the inference would be that the difference between those flyers and a bird is that the latter is able by a natural effort to create a vacuum in some part of its body, so as to neutralise the action of gravity. The position of the gizzard and the immense strength of its sinews seem to make it well adapted for such a purpose.

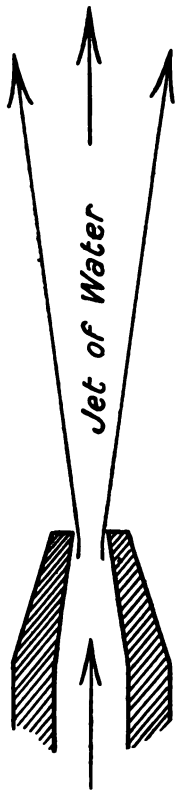
I have said enough in some of my previous letters to show that it would be out of place for me to offer any apology for addressing you on this subject.

I am, my dear Sir,

Yours very truly,

WM. LEIGHTON JORDAN.

DIAGRAM VI.



Section of Nozzle.

APPENDIX V.

THE SPREADING OF THE JET OF WATER.

To Dr. J. H. T. Tudsbery, Secretary, the Institution of Civil Engineers.

29th April, 1907.

My dear Sir,—After hearing Mr. Kellow's paper on hydro-electric power, I obtained a printed copy of it, in order to be prepared to make some remarks on the following Tuesday in case of finding a chance in the rush of speakers; who, however, maintained an interesting flow of discussion without touching the important point on which I venture to comment.

Fig. 5, on page 10 of Mr. Kellow's paper, shows a jet of water issuing from a nozzle; and the author treats the dispersion of the jet, as it flows away from the nozzle, as being due to the difference of velocities at its centre and periphery. That idea clashes with the true basis of all hydraulic power. The fact is that, as water is incompressible, and as its velocity must of necessity decrease as it gets away from the projectile force of pressure inside the nozzle, it must spread so as to give a sectional area increasing as the velocity decreases. The advantage of utilising the force close to the nozzle, instead of at a distance from it, is due solely to the fact of hydraulic power being as the square of the velocity, so that with the decreasing velocity beyond the nozzle the power rapidly decreases.

The author records an interesting experiment in which, by arranging an obstruction in the centre of the nozzle, he reduced the dispersive action. That, however, merely shows that the obstruction must have created an eddy just outside the nozzle; and the decrease of velocity would be just the same at any given distance relatively as in the case of the unobstructed jet, though the eddy neutralises part of the area at the nozzle. The dispersive action is prevented only by wasting power at the nozzle.

I ask the Council to consider whether it would not have been more creditable for our Institution to have avoided printing such an error in fundamental principles of the engineering profession. I do not mean that the paper ought to have been rejected; but that, in view of its merits and importance in other respects, the 'Publication Committee' might have suggested to the author a reconsideration of the point I have specified. Any such friendly hint would doubtless have sufficed to lead him to eliminate the error.

* * * * * *

An important subject, on which I have on several occasions addressed you, has been forced again upon my attention by Mr. Savile's paper on the new railway bridge at Bristol, and Mr. Allan's on the Pyrmont bridge, which were read and discussed at the last two Tuesday meetings. It is rendered all the more appropriate for me to comment on the subject inasmuch as it serves to illustrate the 'personal' element in opposition to new views, to which I have alluded in the letter I addressed to you on 18th instant.

In a paper on iron bridges, read about two years ago, Mr. Marriott describes (p. 7) how some old girders were removed from a bridge and replaced by others of a different construction. It is mere matter of fact that the new form of girder reduces by about one-half the worst of the leverage stresses on the rivets which I had described in a letter which I addressed to you on the 8th March, 1900.

The first step towards getting rid of mischievous stresses on rivets is a knowledge of the mechanical action by which they are caused, and I do not think that action had been clearly defined in anticipation of my above-mentioned letter. At any rate, neither the discussions I had listened to in our Institution, nor any works to which I obtained access, explained them; and (writing from memory) more than one work alludes to the loosening and breaking of rivets in the under side of girders as inexplicable. That was the position when I addressed you on the subject. Of course the new form may have been arrived at quite independently of me; but it was clear that at the time I wrote my letter of 8th March, 1900, the speakers I had heard in discussions and the writers I had read did not then understand those stresses; and the medal, or premium, given by the Council

five years afterwards in recognition of the new form of girder forms a thorough justification of my action in addressing the Council on the subject. This, which is really a very small point in the work I have done in the course of the past forty years, is pulled into light again by the papers on the new Bristol bridge and the Pyrmont bridge. The sections published with the latter paper do not show precisely how the leverage stresses I explained have been guarded against. But they show that the method is not that adopted by Mr. Marriott in the work for which he obtained the Council's medal. As far as I can judge from the published sections, the vertical riveting which resulted in the leverage stresses which I explained has been abandoned altogether and substituted by horizontal riveting, which almost completely annuls the leverage and makes the work depend on shearing stress, which seems pretty certain to be much more effective in withstanding wear and tear. And in the Bristol bridge both methods of avoiding the mischievous stresses appear to have been availed of, as well as I can judge from the paper. At any rate, no vertical riveting appears in the position of that which I criticised seven years ago.

A remarkable phenomenon in connection with this question is that, as far as I have been able to ascertain, the reason for these new forms of girders being superior to those now obsolete is not anywhere explained in the published Proceedings of our Institution. It seems to exist only in my above-mentioned letter, and, indirectly, in the explanation of the same action in my unpublished paper of 21st December, 1900.

I am, my dear Sir,

Yours very truly,

WM. LEIGHTON JORDAN.

The error as regards hydraulic action which the foregoing letter indicates has been published in the Proceedings of the Institution, as if it had not been perceived by anyone present at the discussion!

APPENDIX VI.

RESISTANCE TO THE MOTION OF SHIPS.

To Dr. J. H. T. Tudsbery, Secretary, the Institution of Civil Engineers.

22nd March, 1905.

Sir,—I understood at the discussion yesterday week that the purpose of Lord Brassey's paper was considered to be to discuss the merits of different sizes of ships, and not their shapes. As, however, Sir Wm. White and Professor Biles both alluded yesterday to United States battleships as drawing generally about two feet less than ours of the same displacement, and seemed to consider it indisputable that the lesser draught places them at a disadvantage, and is forced on them, nevertheless, in consequence of the shallowness of their home ports, I hope I may be allowed to add a short contribution to the discussion for the purpose of suggesting that the lesser draught may prove to be really advantageous.

According to the principles originated by the late Mr. Wm. Froude, based on the idea that the inertia of the water does not resist the ship's way, the deepest draught consistent with stability ought to be the most advantageous. But I beg to be allowed to urge that the above-mentioned idea is erroneous and has probably given our battleships the same inferiority to those of the United States, as was proved in the case of our yacht 'Shamrock III.' as compared with the American yacht 'Reliance,' in the recent Cup race.

Suppose, for the sake of illustration, one of two ships of the same displacement to have a mean draught of 11 feet and the other 10 feet. Then, if the water displaced had all to be lifted to make way for the ships going at the same speed, one would have to lift its displacement 11 feet in the same time as the other would have to lift it only 10 feet, and the forces requisite to do the work would be as the squares of those relative velocities,

or, : 122 :: 100. That is to say, the ship with the deeper draught would, for that special purpose, have to expend one-fifth more power than the other.

There can, however, be no doubt, I think, that ocular evidence of the movement of water near a steamer in calm weather and the slip of the screw, show that some quantity of water must reach the stern of any steamer by passing under the ship's bottom with greater velocity than the ship progresses through the ocean. The increase of friction to be overcome in consequence of that action cannot be equivalent to the work saved in lifting the water, as the water will take the easiest route out of the ship's way. But whatever water may sink right ahead of the ship to pass under her, instead of being lifted, must reduce the effective action of the screw by increasing the slip.

The part of the water which is not lifted may therefore be more detrimental to the ship's progress than any equal quantity which is lifted.

I hope this note of warning against the manner in which the idea that the inertia of the water does not resist the ship's way is being given practical application by our shipbuilders, may be allowed to appear in the report of the discussion.

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

To Dr. J. H. T. Tudsbery, Secretary, the Institution of Civil Engineers.

25th March, 1905.

Dear Sir,—The question dealt with in my letter of 22nd instant is of such importance that I think it desirable to submit some diagrams for the purpose of making the points at issue quite clear.

Fig. 1*a* shows two midship sections which, according to statements connected with plate v., fig. 5, in a paper read by Mr. R. E. Froude on the 24th March of last year before the Institution of Naval Architects, I understand to have been found to be subject to the same resistance. According to my argument that shows the increased lift from the area A instead of from the area B to be compensated by reduced friction and greater facility for lifting action. The difference between the lift from

DIAGRAM VII.

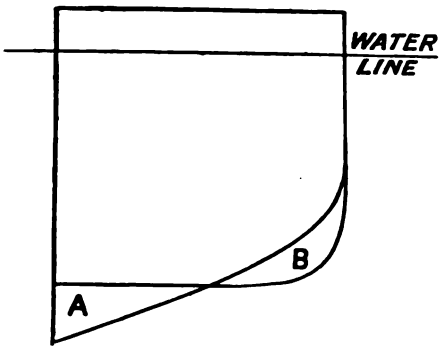


FIG. 1*a*.—Half-Section.

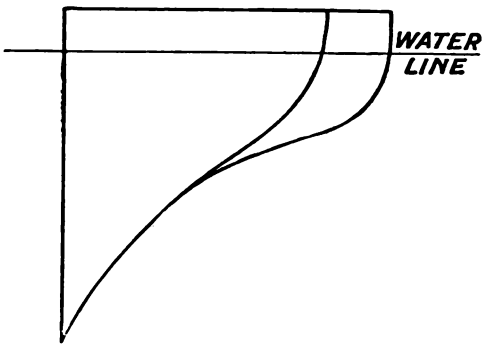


FIG. 1*b*.—Half-Section.

FIG. 2.—Half-Plan at Water Line.

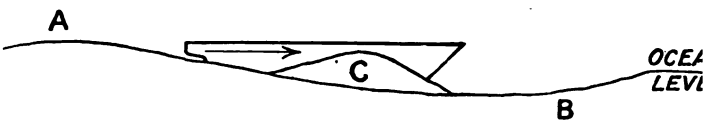


FIG. 3.

A and that from B is not very great. To get the full advantage of a reduced lift the area substituted for B should be added at the water line by extending the maximum beam (or an approximation to it) fore and aft, as shown in fig. 1*b*.

Fig. 2 illustrates an experiment recorded by the late Mr. William Froude in a lecture given in this Institution in May, 1876, which showed that at high speeds the broader beam, though increasing the displacement, made considerably the least resistance. That I treat as being due to the decreased height of lift more than counterbalancing the increased area of displacement.[1]

In fig. 3 A and B represent respectively the crest and depression of the gravitation wave which tends to readjust the equilibrium disturbed by the ship's motion; and C represents the superimposed wave lifted by the pressure of the ship's motion.

I contend that the inertia of the water combines with that of the ship to resist the force which first sets the ship in motion, and that it continues incessantly to resist that motion as long as the latter is in action.

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

'The work of "preparing to build" the "Dreadnought" was begun at Portsmouth early in 1905.'—SIR WILLIAM WHITE 'Times,' 4th August, 1908.

The following letters show how the correction of the erroneous principle adopted by Stokes and Froude originated in the latter part of my letter of 12th January, 1903, written two years before the building of the 'Dreadnought' was commenced.

To the Secretary, the Institution of Civil Engineers.

12th January, 1903.

Sir,—My letter of 12th November last commented on the official attitude of the Institution of Civil Engineers as regards the reformation of the laws of motion which I advocate. I had in previous letters pointed out how, whilst the Institution are

[1] See letter of 21st December, 1903, on page 68.

giving the weight of their prestige and authority to the laws formulated by Newton under the guidance of the opinions of Galileo and Descartes, some of the most renowned members of the scientific world have practically discarded the main point which I publicly attacked in 1866.

I wish now to point out that Professor Hele-Shaw, who is not only a member of the Institution of Civil Engineers, but is also Professor of Engineering in University College, Liverpool, has in a series of lectures just recently delivered in this Institution treated the theory of motion in accordance with the reformed method I have advocated, tacitly ignoring that part of the so-called First Law of Motion which I have pointed out to be obsolete. He lucidly showed how obstructions on a road prevent wheels from following the straight line in which they tend to move, and that motion is spent because work is done in overcoming the obstacles which prevent motion in a straight line; and afterwards, rolling a wheel in a circle, he said: 'The wheel continues in motion until the energy given to it is exhausted: that is the First Law of Motion.'

It is mere matter of fact that the views expounded by Professor Hele-Shaw throughout his lectures constitute an abandonment of Newton's First Law of Motion and an adoption of the law which I have substituted for Newton's. The fact that the title of the lectures was, 'Locomotion—on the Earth, through the Water, in the Air,' and that the lecturer throughout kept clear of astronomical questions, cannot properly be considered to affect the subject; for, though locomotion in interstellar space involves other conditions under which the law has to act, 'the First Law of Motion,' whatever it may be, must of necessity be the same First Law independently of the conditions under which it may have to act.

According to Newton's law the orbital motion of the earth is ceaselessly doing the mighty work of overcoming the ceaseless effort of the sun's gravitation to draw the earth down to the sun's surface. That effort of the sun's gravitation is the 'obstacle' which prevents the earth from following a straight line, and compels it to move in a curve; but nevertheless, according to Newton's law, that immense amount of work is done every instant by the earth's motion without renewal of motive energy, and without reduction of the amount of energy of motion im-

parted under conditions no longer existent; whereas according to the law expounded by Professor Hele-Shaw the work done by the earth's motion must involve expenditure of the motive force, which must therefore gradually be spent unless ceaselessly renewed in proportion as work is done by it. That ceaseless renewal of motive force is, I say, supplied by the transverse action of the sun's gravitation, of the existence of which Galileo, Descartes, and Newton were unconscious, and, therefore, invented a law of motion to supply in theory the incessant force found to be practically necessary to maintain the earth's motion. I have (in a paper on 'Vis Inertiæ and Momentum,' offered to the Institution some years ago and since published in my work 'Essays in Illustration of the Action of Astral Gravitation') so fully commented on the circumstances which led to the invention of that so-called First Law of Motion, that I will not now repeat arguments, but pass to a new aspect of the subject.

Though Professor Hele-Shaw completely ignored the obsolete idea of any body in the material universe having an innate tendency to continue in perpetuity any motion imparted to it by an initial push or impulse, he nevertheless, in one of his lectures, expounded a commonplace phenomenon in a manner which is a survival of a method of explanation introduced under the influence of that obsolete idea.

In an exceedingly interesting and lucid lecture on 'The Self-propelled Ship,' he made the threefold division of the resistance to a ship's motion—(1) skin friction; (2) wave-making; and (3) eddy resistance, which was formulated by William Froude, who will hereafter perhaps be recognised as the last thoroughly earnest and uncompromising champion of that part of Newton's law which I have declared to be obsolete. His thorough belief in that idea as an active principle in nature led him to endeavour to direct in accordance with it the designing of the forms of ships best suited for moving through the water under its action. Thoroughly believing that fancy to be a really effective and controlling action in insterstellar space, he exerted his ingenuity to the utmost to utilise the principle in devices for propulsion through the water.

I do not say that there is anything intrinsically wrong in that threefold division of resistances, provided they are not treated inconsistently with the fact that they are subdivisions of the

action by which the inertia of the moving material resists the force by which it is moved; and certainly Professor Hele-Shaw was not in his lectures misled into what appeared to me any mistake by adopting Froude's method; but Froude systematically ignored the fact that the 'waves' and 'eddies' he deals with are the motion by which the water makes way for the ship. The inertia of the water, which, as it is forced aside to make way for the ship forms waves and eddies, is expressly denied by Froude to constitute a resistance to the ship's way ('Proceedings of the Royal Institution,' May 12th, 1876, p. 211). He is enabled to ignore the resistance due to the inertia of the water only because he divides it under three separate heads and ignores the fact that those three resistances are consequences of the ship having to force the water aside to make way for itself, and that they are the effective actions in the effort by which the inertia of the water resists the effort made to move it. Work is done in moving the water to make way for the ship, and motive force must be expended in doing that work. But Froude not only believed the earth's motion to be perpetuated by inertia, but endeavoured to contrive a utilisation of the idea in the construction of ships. Liberal arrangements by the Admiralty for practical experiments with models, together with joint responsibility of colleagues, has probably prevented some strange shapes from being launched from our building yards whilst Froude was endeavouring to establish his idea; and no doubt, in spite of unsuccessful attempts to develop erroneous theory, useful work has been done, and correct views on some points not previously recognised have become established. About seven years before the date of Froude's above-mentioned paper I was criticised in Buenos Ayres for removing outriggers from a London wherry and fixing them on a Rob Roy canoe for a race in which I undertook to pull a pair of sculls against a four-oared gig over a course of twenty miles. The argument against me was that I was taking a slower boat; but my counter-argument was to the effect that though the wherry was the best for a short race, no man could pull it for twenty miles at the average speed at which he could pull the Rob Roy; and I notice that my view on that point is elaborately justified by Froude's argument in connection with diagram No. 22 in his paper. At any rate, I pulled over the course in the Rob Roy in less time than I con-

sider I could have done in the wherry. In questions as to the total amount of resistance to any given vessel Froude was no doubt correct notwithstanding his idea that the inertia of the water does not resist the progress of a ship passing through it.

The idea that one initial impulse can be the cause of perpetuity of motion in interstellar space has doubtless led to hours, days, and years having been often spent in the endeavour to contrive a similar perpetuity of motion by mechanical arrangements in terrestrial phenomena by arguments perhaps in some cases sound, supposing their basis to be sound, and Froude's arguments have originated in that same false basis.

I again urge on the Council of the Institution of Civil Engineers that the question I have raised regarding the First Law of Motion is one of vast importance in engineering science. And I also urge that this letter must properly be considered to originate in that of 21st December, 1900, which was written for the immediate purpose of pointing out that the true cause of the accident to the Great Northern Express, which had just then been the subject of a Board of Trade report, had been missed by those who framed that report.

I am, Sir,

Yours truly,

WM. LEIGHTON JORDAN.

Assoc. Inst. C.E.

To the Secretary, the Institution of Civil Engineers.

24th January, 1903.

Dear Sir,—In your letter of 20th instant you allude to mine of 12th *idem* as generally reiterating views expressed in previous communications to which answers have already been made under the Council's instructions. I have already, under date of 21st instant, replied to some points of your above-mentioned letter, and now earnestly urge upon the Council some observations in protest against the incorrect statement I have specified, and which is the really important point of your letter.

It seems to me impossible that any one could read the letter to which you reply without recognising that the point which it professes to treat as of the greatest practical importance is the exposition it gives of the error at the basis of the theoretical

principles by which William Froude was guided in designs which more or less determined the lines of many ships of our Navy whilst the influence of the views which he introduced predominated in the Admiralty.

You profess to be refraining on your sole responsibility from submitting that letter to the Council, on the ground of the views it expresses having been previously replied to by them ; but the fact is that the exposition there given has never before been offered to you either by me or by anyone else. The Council have never replied to it, and have never had an opportunity of considering it except that given by my letter of 12th instant.

Froude's influence in the Admiralty originated in his opposition to the reformation of the laws of motion which I suggested, and the shapes of many of our battleships and cruisers are a testimony of the error of that part of the so-called First Law of Motion which he endeavoured to make available for practical purposes after I had declared it to be obsolete as a theory and false as a practical principle in all mechanical action.

Every ship now afloat in our Navy constructed, under the guidance of Froude's ideas, with ' hollow lines ' is a practical demonstration of the fact that Newton's definition of inertia, which Froude endeavoured to make practically effective, is not a true principle in nature.

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

Assoc. Inst. C.E.

To Dr. J. H. T. Tudsbery, Secretary, the Institution of Civil Engineers.

21st October, 1903.

Dear Sir,—As I am confident that the attitude of the Council indicated by your letter of 3rd July is due to a conviction on their part that I am wrong as regards the main point of the question I have urged upon them, and as I am also confident that they cannot be imbued with any desire to refrain from allowing fair play to new views even when clashing with theory long accepted as a basis for the discussion of engineering questions, I therefore consider it a duty to myself and to the Council, as well as to the true interests of engineering science, to disregard their

request and to reinforce my argument to the effect that every ship in our Navy recently designed under guidance of the principles which led to the form to which some of our admirals have recently applied the term 'hollow-lined,' constitutes a practical demonstration to the effect that what is now accepted in all engineering text-books as the First Law of Motion is not true to nature.

The guiding theory which has been followed as much as practical experiments with models permitted not only discards the previously accepted basis of discussion to the effect that the work to be done by a ship, sailing or steaming, is to excavate a passage for itself along the surface of the water, but also, instead of that idea as a basis for treating the subject, the recently adopted theory is based on the idea that 'a fish moving at a steady speed through frictionless fluid would experience no resistance at all. In fact, when once put in motion it would go on for ever without stopping.'

I am not at all surprised that anyone who thoroughly believes that the earth has been for thousands of years moving round the sun at the same velocity, in consequence of an initial impulse, without any force being now incessantly in action to sustain the motion, should attempt to utilise that idea in the manner indicated in the above quotation.

It is argued that the inertia of the water offers no resistance to the progress of the ship, because the head resistance as the water is pushed out of the ship's way is balanced by the stern pressure of the water refilling the vacuum. Granting the argument to be correct as far as the fore and aft pressures are concerned, I say that, even in the assumed frictionless fluid, the stern pressure represents the force which propels the ship, and any reduction of that propelling force leads to a corresponding reduction of the stern pressure. The force with which a stone lifted vertically strikes the ground on its return is equivalent to the force exerted to lift it; but there must be fresh expenditures of lifting force to enable the stone to continue striking; and so also the force which any drop of water falling back to its place exerts as stern pressure must first be supplied by expenditure of force exerted in lifting it from its place.

The protests which have come from practical seamen against the shapes of some of our new ships show that for the comfort

of our sailors at all times, and for the accuracy of our fire when fighting, the dominant idea by which the Admiralty have been recently guided, and are still avowedly guided, in naval construction ought to be abandoned; and it appears to me that those principles are also erroneous when considered only as a means for obtaining a maximum of speed with a minimum of resistance.

The term 'venom of pitching' was probably unknown in the Navy before William Froude and Scott-Russell answered my attack on the First Law of Motion by giving it more practical application in naval construction than had ever before been attempted.

I am, dear Sir,
Yours truly,
WM. LEIGHTON JORDAN.
Assoc. Inst. C. E.

To Dr. J. H. T. Tudsbery, Secretary, the Institution of Civil Engineers.

21st December, 1903.

Dear Sir,—As I am confident that the following arguments will be found to justify me in expecting that the Council will change from the attitude reported in your letter of 27th November as regards my effort to impress on them evidence to the effect that the credit of engineering science and the interests of the country demand a radical change in the theoretical principles by which the Admiralty have in recent years been guided in naval construction, I urge them on your attention, premising that it has been, I presume, understood that I do not pretend to determine precisely the shapes which correct principles require. William Froude (in the paper referred to in my letter of 12th January) admirably comments on the point I am bringing to issue when he says: 'It is easy to imagine how fruitful, in false aims and false principles of nautical construction, would be the assignment of the resistance of ships to a supposed cause which has no existence at all.'

It must, I think, be admitted that it may be equally disastrous to design the shape of a ship on the supposition of the non-existence of a resistance which in fact does exist; and that, I say, is what has been done during the past quarter of a century and is now avowedly being persisted in. The merits of the point

at issue depend on the question as to whether the inertia of the water through which the ship moves does or does not cause it to resist being displaced to make way for the ship. That main point I have directly dealt with in my letters of 12th and 24th January and 21st October, and I will now support that argument with the following points.

First, in the paper above referred to, Froude says: 'I am but insisting on views which the highest mathematicians of the day have established irrefutably, and my work has been to appreciate and adapt those views when presented to me.'

I agree that if the inertia of matter really tended to perpetuate motion in accordance with the axiom adopted by the mathematicians above alluded to, it might be mathematically demonstrated that the inertia of the water through which the ship moves does not resist the ship's way; but I urge that, in fact, the logical application which has been made of that principle in naval construction constitutes a practical refutation of the principles on which those mathematical demonstrations have been based.

In diagram 23 [1] Froude gives water-line plans of two ships of the same length, but one with more beam than the other; and he asserts that, 'on the midship section theory this one would clearly have the greater resistance of the two.' The context shows that the illustration is given in contravention of the idea that the resistance is due to the inertia of the mass of water which has to be displaced. That mass is identical with the mass of the ship; and I say that the question at issue in those two shapes is dependent not only on the mass and velocity of the ship, but also on the relative amount of work which has to be done to get equal masses of water out of the way of ships of different shapes moving at the same velocity.

Mr. Froude continues: 'Nevertheless, in the trial of two models of those lines it appeared that at the higher speeds the form with the largest cross section made considerably the least resistance.' And he then argues that 'the explanation of this lies of course in the fact that the addition amidships, though increasing the displacement, forms a prolongation of the wave features at the two ends, and thus lessens the wave-making resistance.'

[1] See fig. 2, diagram VII.

In reply to the above, I say, in the first place, supposing the two ships to be of the same displacement, the broader beam offers least resistance, because the displacement is closer to the surface and therefore more easily affected. And, secondly, it is an interesting and important question to determine to what extent the greater facility of displacing water near the surface than deeper will admit of a greater tonnage being moved at equal velocity without greater resistance. It seems to me that the 'increased displacement' referred to by Froude cannot have been very great. And I say the evidence of the experiment really tends to show that the ship's motion causes a wave only because that is the easiest method for the water to get out of the ship's way. The deeper the displacement, the greater is the height through which the water has to be lifted to make way for the ship. This seems to me to have been exemplified by the manner in which 'Reliance' beat 'Shamrock III.' in all weathers. I have not found any practical experiment recorded to show that resistance is greater at the surface than deeper, as asserted by Froude and others, and in the absence of such demonstration it seems to me that the idea ought not to be relied on as a fact on which to base argument. The wave rises to permit the ship to advance, and if the ship could progress more easily without raising the wave, the wave would not be created. As water is practically incompressible under the conditions we are discussing I do not understand how it can be supposed to make way for the ship (whether totally or only partly submerged) without rising in some form of wave. It seems to me that mathematics which demonstrate the possibility of so shaping a ship as to avoid the creation of a wave must of necessity be based on some principle which is not true to nature.

Other circumstances which justify the insistence of my appeal to the Council of Civil Engineers are that the result of the recent discussion in the Institution of Naval Architects was a declaration by the President to the effect that Admiral Fitzgerald, who protested against 'hollow lines' as practically disadvantageous, had been defeated; and the discussion shows no intention of changing from the theoretical principles by which William Froude, Scott-Russell, and Nathaniel Barnaby were guided in designing the shapes against which Admiral Fitzgerald

protested. Admiral Fitzgerald's protest is based on his practical experience at sea on board the ships. He simply declares the shape of the ships to be bad without entering on the question as to whether that is due to erroneous principles of construction or to faulty treatment of scientific principles themselves intrinsically faultless. My argument is, on the other hand, merely to the effect that the principles by which the Admiralty have avowedly been guided are erroneous. And that is a question independent of that as to the extent to which tank experiments have prevented practically disastrous shapes from being constructed. The argument I am urging is untouched in the discussion of the naval architects or anywhere except in the appeal I have made to the Council of the Institution of Civil Engineers.

I beg, however, to be allowed to point out that the above-mentioned discussion seems to have turned more upon what is a trivial point than on the main question even from Admiral Fitzgerald's point of view. It might be supposed from the tenor of the discussion that all at issue in the protest was about fifteen inches of width on each side of part of the bows of those huge ships. But, in fact, that is merely the extent to which at the water-line the bow curves inwards. Lower down the inward curve is more than three feet; and the real question at issue is as to whether the bow ought not to bulge *outwards* two or three times as much as it is actually made to bulge inwards.

The question really raised by Admiral Fitzgerald is, therefore, as to whether there is not a width of twenty-five feet *outside* some parts of the bow which might advantageously be brought inside by constructing a bluff instead of a hollow lined bow. That is to say, the question is as to whether the line from the cutwater to the full beam of, say, sixty-six feet is in some part to place a width of twenty-five feet inside or outside the ship; and that constitutes an important area even in the largest ships. That question does not, however, affect my argument as to the merits of the theoretical principles which the Admiralty are endeavouring to make effective; and the fact of the discussion by the Institution of Naval Architects resulting in favour of the maintenance of the principles of which Froude was essentially the champion demands persistence in the crusade against them.

The Council I am addressing are pre-eminently qualified

to deal with the twofold question which I have propounded, and I trust the immediate reason given in previous letters for appealing to them will not be ignored.

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.
Assoc. Inst. C. E.

To Dr. J. H. T. Tudsbery, Secretary, the Institution of Civil Engineers.

13th April, 1904.

Dear Sir,—In my letter of 21st December last I urged, in justification of my insistence in addressing the Council of the Institution of Civil Engineers on the subject of naval construction, the fact that the last discussion by the Institution of Naval Architects had resulted in favour of the maintenance of the principles of which the late William Froude was essentially the champion.

A series of lectures which Lord Rayleigh has recently delivered in this Institution on ' The Life and Work of Stokes ' constitutes a further demand for persistence in my crusade against those principles.

Lord Rayleigh claims that it is only by practical application of the mathematical work of Stokes that Froude and others established the principles of naval construction which have been adopted by the Admiralty. The whole tenor of the argument in which Lord Rayleigh claimed credit for Stokes for the principles by which the Admiralty have been guided in designing the shapes of recently built ships, is a thorough confirmation of the argument of my letters of 12th and 24th January and 21st October, 1903, as far as concerns my statement that ' Froude's influence in the Admiralty originated in his opposition to the reformation of the laws of motion which I suggested.' Stokes considered my opposition to the ' First Law ' to place me outside the pale of reasonable science ; and Froude evidently considered that a mistake had been made, but that it consisted, not in any error in the laws of motion, but in never having given the First Law its due practical application in designing the shapes of ships. It appears to me that Lord Rayleigh is quite correct in claiming that the designs of our recently built ships have been based on

e mathematics of Stokes; and no scientific man versed in the bject will, I think, dispute that in the principles which Stokes presented and acted on, that First Law was regarded as an qually reliable axiom as the statement that two and two are four.

The point I wish to enforce is to the effect that the shapes of the ships under discussion are the result of an attempt on the part of practical men to put into practice principles demonstrated by pure mathematicians to be correctly based on what they suppose to be incontrovertibly the true laws of motion. Lord Rayleigh has, in fact, fixed on Stokes the responsibility, according to the tenor of my letters above mentioned, for 'Shamrock III.' having been so shaped as to give her no chance in any weather against 'Reliance.' It seems to me that the American builder was guided by experiment rather than by theory in changing from the shape of 'Defender,' and perhaps did not clearly recognise the precise theoretical points which made his new boat superior to ours.

I wish to avoid repetition of arguments already laid before you; but I am able to reinforce those arguments by the evidence of a practical experiment recently made in the Lecture Hall of this Institution.

On a day intervening between two of the above-mentioned lectures by Lord Rayleigh, Professor Trouton delivered a lecture in which he exhibited an experiment in illustration, professedly, of the manner in which Stokes's mathematical inductions from the laws of motion had been applied to shipbuilding as described by Lord Rayleigh.

In the diagram (page 74) A and A′ are two similar glass vessels containing liquids; the liquid in A having greater specific gravity than that in A′.

B and B′ are two bubbles started at the same time from the bottom of the respective vessels.

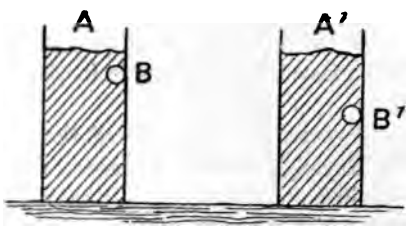
Professor Trouton argues that the slower motion of the bubble B′ illustrates the 'eddy resistance' to the motion of ships, because the liquid in the vessel A′ (which is in fact less dense than that in A) eddies behind the rising bubble B′ more than is the case with the liquid in the vessel A as regards the bubble B; and that in consequence of the greater eddy resistance the upward motion of B′ is more retarded than that of B.

I must say it appears to me that Professor Trouton is perfectly justified in offering that experiment as an illustration of

Froude's theory of eddy resistance to the motion of ships; but I beg that, as practical men, the Council of our Institution will recall to mind Torricelli's original explanation of the rising and falling of the common barometer, and recognise that the principle then established naturally requires the bubble B to rise faster than B′ simply because the liquid A being more dense than A′, a greater force is applied to propel B upwards than is applied in the same manner to B′. The force of propulsion is the amount by which the earth's gravitation acting on a volume of liquid equal to that of the bubble exceeds that acting on the bubble; so that the propelling force acting on B exceeds that acting on B′ exactly as the density of the liquid A exceeds that of A′.

There doubtless is some difference in the formation of eddies behind the two bubbles, due to what Lord Rayleigh terms viscocity, and which I treat as chemical affinity, whether a bubble

DIAGRAM VIII.



or a ship is under discussion (friction being as the degree of chemical affinity); but in giving that as the cause of the difference in the velocity of their motion, theoretical guesswork, based on theoretical action of theoretical laws, is being substituted for a plain matter of fact explanation which had been given long before the theoretical laws or the still more clever and elaborate mathematics based upon them had been applied to naval construction. I have, in previous letters, pointed out that in the practical work of naval construction Froude has made many of our finest ships practical proofs to the effect that the principles accepted by Stokes are not true to nature; and it seems to me that Professor Trouton, in his explanation of the above-mentioned experiment, has very cleverly brought those principles to a *reductio ad absurdum*.

In face of the influential position of the Council of the Institution of Civil Engineers, a great responsibility will rest on them and lessen the high estimation in which they have deservedly been held if in spite of the facts and arguments I have submitted to them any more vessels are built in our dockyards under the principles of construction which Froude based on the mathematics of Stokes. But a responsibility of a quite different and of a still more serious character will be incurred if all members of the scientific world remain impassive whilst the deliberately unfair conduct against which I have protested is persisted in whilst the Admiralty abandon the false principles of Stokes and Froude and act on those for advocating which I have encountered the hostile action described in my letters of 29th April, 1901, 17th May, 1901, 3rd November, 1902, 25th February, 1903, and 12th May, 1903.

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

Assoc. Inst. C. E.

To Dr. J. H. T. Tudsbery, Secretary, the Institution of Civil Engineers.

9th January, 1905.

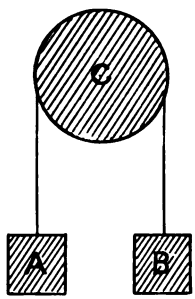
Dear Sir,—I have received from two independent sources fresh supplies of fuel for urging my appeal to our Council, as representatives of the most practical men among those prominent in science, to rescue the scientific world from the predicament in which it has been placed by those in possession of the Royal Society.

First, recent lectures in this Institution: A course of six lectures has just been given on 'Measuring Time'; but their main purpose was, at the outset of the first of them, avowed to be to impress on the minds of a juvenile audience the truth of the laws of motion declared to have been discovered and irrefutably established by Galileo and Newton.

One of the two phenomena declared to be a practical proof of the so-called First Law was shown by 'Atwood's machine,' which has been utilised for the purpose for more than a hundred years.

The point essential to this question is that equal weights, A and B, are suspended over a pulley C; a slow downward motion is artificially given to B, with corresponding upward motion to A; and then, because the motion so created is continued after the action of the force by which it was started is removed, the juveniles are told (as the lecturer was himself told when he was a juvenile) that they see before them a practical demonstration of the truth of the law which declares that a body once set in motion tends to persist for ever in that state of motion. The glaring facts of the case are that, if the weights are really in equilibrium when on a level, as in the diagram, then any fall of B and rise of A must make B heavier than A, because the respective weights are the respective forces of gravity acting

DIAGRAM IX.



between each of them and the earth, so that the motion increases the weight of B and decreases that of A.

The fact is that motion is continued in the above experiment simply because each quantity of motion effected brings into action a fresh amount of force for the creation of a further quantity of motion, even without considering the weight of cord transferred from one scale to the other as the motion is effected.

The motion continues because motive force is continuously supplied.

The other experiment in asserted demonstration of that so-called First Law was a dummy passenger placed in an open railway truck; and on the truck being suddenly stopped by an obstruction on the rails, the passenger went head first forwards out of the truck. As far as exemplifying that first law is concerned, a bullet might have been fired from a rifle or flung from

a sling, and the motion beyond the barrel of the rifle, or after leaving the sling, declared a proof of the law.

The fact is that in each case a quantity of motion proportioned to the motive force exerted on the propelled body is effected and motion ceases as the force is spent. For continuity of motion there must be continuity in the supply of motive force.

A phenomenon alluded to which really does bear on the above question, though it was not treated as being connected with the subject, is the fact that to make an ordinary pendulum truly isochronous with changes of amplitude its path must be made to describe a segment of a cycloid instead of that of a circle.

The only explicit explanation that has ever been given of that fact is that the action of astral gravitation resists all motion with a force which is as the square of the motion resisted; and the slow motion in the short vibrations of the pendulum is therefore less resisted than the faster motion in longer vibrations. To compensate for that decrease of resistance in short vibrations a seconds pendulum has to be made about half an inch longer if used in them than if used in vibrations of 90° (as shown in my letters of 7th May and 21st November, 1903).

The existence of that resisting action makes it impossible for continuity of motion to exist in the universe without continuity in the supply of motive force; and the so-called First Law is therefore useless as an explanation of any existing continuity of motion. Some other cause must in each case exist.

The second source to which I have alluded is a series of letters recently addressed to the 'Times' by Sir William White on the United States navy.

Sir William White tells us that United States battleships of the 'Vermont' class displace, when fully loaded, 17,650 tons and draw 26¾ feet; and ours of the 'King Edward' class displace 17,500 tons and draw about 28½ feet when similarly equipped.

That difference between our battleships and those of the United States is exactly what I suggested (in my letter of 21st December, 1903) as being probably the difference between our yacht 'Shamrock III.' and the American 'Reliance,' which gave the latter the superiority both in steadiness and speed.

I am not going to rewrite the arguments of my letters of 12th January, 1903; 24th January, 1903; 21st October, 1903; 21st December, 1903, and 13th April, 1904, to the effect that all our hollow-lined cruisers or battleships now afloat are practical

demonstrations to the effect that the so-called First Law of Motion is not true to nature, but merely recapitulate to the effect that up to the time of my attack on that law, its direct application to shipbuilding had never been attempted. The late William Froude then, instead of rejecting the law, as I proposed, gave it practical application under guidance of the mathematical demonstrations of Sir G. G. Stokes and others, and the result which now becomes evident is that our battleships have to lift the water from nearly two feet of greater depth to make way for themselves through the ocean than has to be done by United States ships of the same class.

Our ships are condemned to do that greater depth of ploughing merely because, under the guidance of Stokes's mathematics, Froude was convinced that the *inertia* of the water does not resist the ship's way.

More work has to be done to lift water 28½ feet than to lift it 26¾ feet; and I leave it to Sir William White to say if the shapes of our ships give them any compensating advantages.

It is time for the Royal Society to recognise that the rejection of my paper on the tides in the year 1868, under the guidance of Sir G. G. Stokes, was a blunder; and Froude's action in applying the so-called First Law to naval construction after I had denounced it was a natural sequel to that blunder.

That law was introduced into science for astronomical purposes; and until Froude applied it to naval construction I do not think any attempt had been made to give it practical application in terrestrial physics except by those whose thorough acceptance of it as a firm theoretical basis for practical mechanics misled them into schemes for endeavouring to contrive perpetual motion by the application of a merely initial force. The resistance of astral gravitation, which limits the quantity of motion to the quantity of motive force, baffled all those clever contrivances.

Sir William White is among those to whom this letter is addressed, and there is no one more competent than he is to decide as to whether the cost of converting some of our ships from hollow-lined to bluff bows would or would not be compensated by the advantages of increased steadiness as fighting platforms and increased speed with equal motive force.

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

To Dr. J. H. T. Tudsbery, Secretary, the Institution of Civil Engineers.

30th October, 1906.

Dear Sir,—In a leading article yesterday the 'Times' raises the question as to how far the 'very high speed' of the 'Dreadnought' may have been obtained at a cost in excess of its value as estimated in terms of fighting efficiency; and then says, 'The design of the "Dreadnought" is not the outcome of any single man's ideas."

I beg most respectfully to urge on the Council that the letter I addressed to you on 12th January, 1903, is the origin of the change from the shape of the 'King Edward VII.' to that of the 'Dreadnought.'

My said letter of 12th January, 1903, and those which followed it on the same subject gave a death-blow to William Froude's 'fundamental Principles of the Resistance of Ships,' by which the Admiralty were guided in the construction of the 'King Edward VII.' and all our other battleships and cruisers then in commission; and the Admiralty are now shaping our ships on new designs based on the mathematical principles advocated in the letters I have addressed to our Institution.

That new mathematical basis has virtually been entrusted by me to the Council of the Institution of Civil Engineers, and I appeal to them not to acquiesce in its appropriation by highly paid official experts without any recognition of the 'amateur' to whom they are indebted.

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

APPENDIX VII.

STEEL RAILS.

To Dr. J. H. T. Tudsbery, Secretary, the Institution of Civil Engineers.

24th February, 1906.

Dear Sir,—A lecture delivered last evening in this Institution by Professor J. O. Arnold, of Sheffield, constituted an elaborate practical demonstration of the correctness of the main point of the second section of the paper which I offered to the Council of our Institution on 21st December, 1900.

Through eight pages of argument I challenged the then prevalent idea to the effect that lines of cleavage in the crystals of any metal are a source of weakness, and against that view I suggested that 'the crystal growth is the effort of gravitation to give the metal its strongest possible conformation, and those views indicate that the effort of manufacturers should be to create uniform crystallisation, so as to make one homogeneous crystallisation pervade the whole rail. The lines of cleavage doubtless are, when homogeneously extended, a source of elasticity in the metal, but I contend that they nevertheless give the strongest bond of union of which the atoms of the metal are susceptible.'

The experiments explained by Professor Arnold are a clear and splendid confirmation of the theoretical argument which I submitted to our Council more than five years ago.

I am, dear Sir,

Yours truly,

WM. LEIGHTON JORDAN.

To Dr. J. H. T. Tudsbery, Secretary, the Institution of Civil Engineers.

26th March, 1906.

Dear Sir,—James Mansergh, who was President of our Institution when the Council rejected my paper of 21st December, 1900, has gone out of reach of the appeal for reconsideration which is overwhelmingly justified, as far as I am concerned, and at the same time urgently demanded from the point of view of the 'interests of the Institution' which the Council, by your letter of 15th December last, have thought proper to inform me it is their object to promote.

Seldom, in the short space of about five years, has a purely theoretical argument been so splendidly confirmed by practical experiments as the main argument of the second part of my above-mentioned paper has been by the demonstrations alluded to in my letter of 24th February. In face of the practical experiments recorded by Professor Arnold 'the interests of the Institution' clearly call for a change of attitude on the part of the Council as regards my paper of 21st December, 1900. The difficulties inseparable from new and abstruse subjects must often be held to excuse misappreciation of papers offered to the Council; but Professor Arnold's lecture in this Institution, referred to in my letter of 24th ultimo, creates a perfectly new position and gives the theoretical action on which that paper is based more importance, and fairly entitles it to greater consideration than if the paper had been written for the purpose of explaining the facts discovered by Professor Arnold after they had been recorded.

I am, dear Sir,
Yours truly,
WM. LEIGHTON JORDAN.

If there were such things as mathematical anomalies, the gravitation formulæ recorded in my letter of 18th January might be treated as such. The fact, however, that 16.1 as the foot-pound-second equivalent of force in gravitation measure gives, as representing g, a complete set of simple formulæ which is not given by 32.2, proves the latter figure to be false. And, moreover, it must be borne in mind that I, in the first instance, pointed

out that the movements of the planets show 16.1 (and not 32.2) to be the true figure, and the subsequent evolution of the formulæ simply confirms the antecedent demonstration to that effect.

W. L. J.

To Dr. J. H. T. Tudsbery, Secretary, the Institution of Civil Engineers.

10th April, 1906.

Dear Sir,—Your letter of 5th instant has only just reached me, having apparently been posted last evening, leaving me only just time to reply before the closing of the session to-night.

As no modification of Newton's gravitational theory can be said to have become generally accepted, and as for forty years there has been no more thorough supporter of the Newtonian law of gravitation than I am, your letter creates a false issue whilst it ignores points really in my correspondence with which it is impossible for anyone to deny that engineering considerations are concerned.

What gives a cynical aspect to the issue created by your letter is the fact of Sir Alexander Binnie being not only President of the Institution of Civil Engineers, but also a member of the Committee of Managers of this (the Royal) Institution. He is therefore one of those to whom letters addressed to Sir William Crookes as Secretary of this Institution are in fact addressed; and the following comment on a lecture delivered in this Institution last May is therefore addressed to him. In a letter dated 15th May I wrote: 'The lecturer's argument to the effect that Newton's law of gravitation requires modification shows him to be completely on the wrong track.'

I beg that you will kindly submit this letter to the Council this evening.

I am, dear Sir,

Yours truly,

Wm. Leighton Jordan.

To Dr. J. H. T. Tudsbery, Secretary, the Institution of Civil Engineers.

16th November, 1906.

My dear Sir,—I have read with much pleasure your letter of 14th instant, which seems to offer the new views I have advocated on several subjects connected with the engineering profession a new platform from which to assail what appears to me to be generally accepted mistakes.[1]

As regards the last sentence of your letter, which seems to rebuff beforehand my availing of the new position offered, I have already advised you that the editor of the 'Engineer' (*ex uno disce omnes*) told me that if he were to admit into his paper my view to the effect that the so-called First Law of Motion is not true to nature, he would make himself the laughing stock of the engineering profession, and that, therefore, my only course is to appeal direct to the leaders of the profession of whom that editor and others stand in awe.

I am, my dear Sir,

Yours very truly,

WM. LEIGHTON JORDAN.

[1] Unrealised up to 18th September, 1908.

TABLE OF STRESS IN DIAGRAM III., *p.* 26.

	Cantilever Weight in Tons		Stress for 1 Foot Vertical
Main posts	170 × 0		0
Panel			
No. 1	1,280 × 28	feet =	35,840
,, 2	1,080 × 74	,, =	79,920
,, 3	920 × 140	,, =	128,800
,, 4	780 × 197	,, =	153,660
,, 5	670 × 253	,, =	169,510
,, 6	590 × 309	,, =	182,310
,, 7	530 × 365	,, =	193,450
,, 8	500 × 422	,, =	211,000
,, 9	470 × 478	,, =	224,660
,, 10	440 × 534	,, =	234,960
	7,430 tons		1,614,110
Girder Panels			
No. 1	200 × 590	feet =	118,000
,, 2	200 × 647	,, =	129,400
,, 3	220 × 703	,, =	154,660
,, 4	70 × 759	,, =	53,130
Cantilever and girder	8,120 tons		2,069,300
Part of large traveller	600 × 500	feet =	300,000
Small traveller	200 × 730	,, =	146,000
Rolling stock and unplaced material	100 × 703	,, =	70,300
And	200 × 534	,, =	106,800
Total weight	9,220 tons		2,692,400

Stress on main posts of cantilever $= \frac{2692400}{315} = 8{,}546$ tons

Stress on the middle panel of the anchor span $= \frac{2692400}{140} = 19{,}230$ tons

N.B.—The weight of the cantilever arm given above is taken from official statements; but its distribution is merely *pro forma*, to illustrate the argument.

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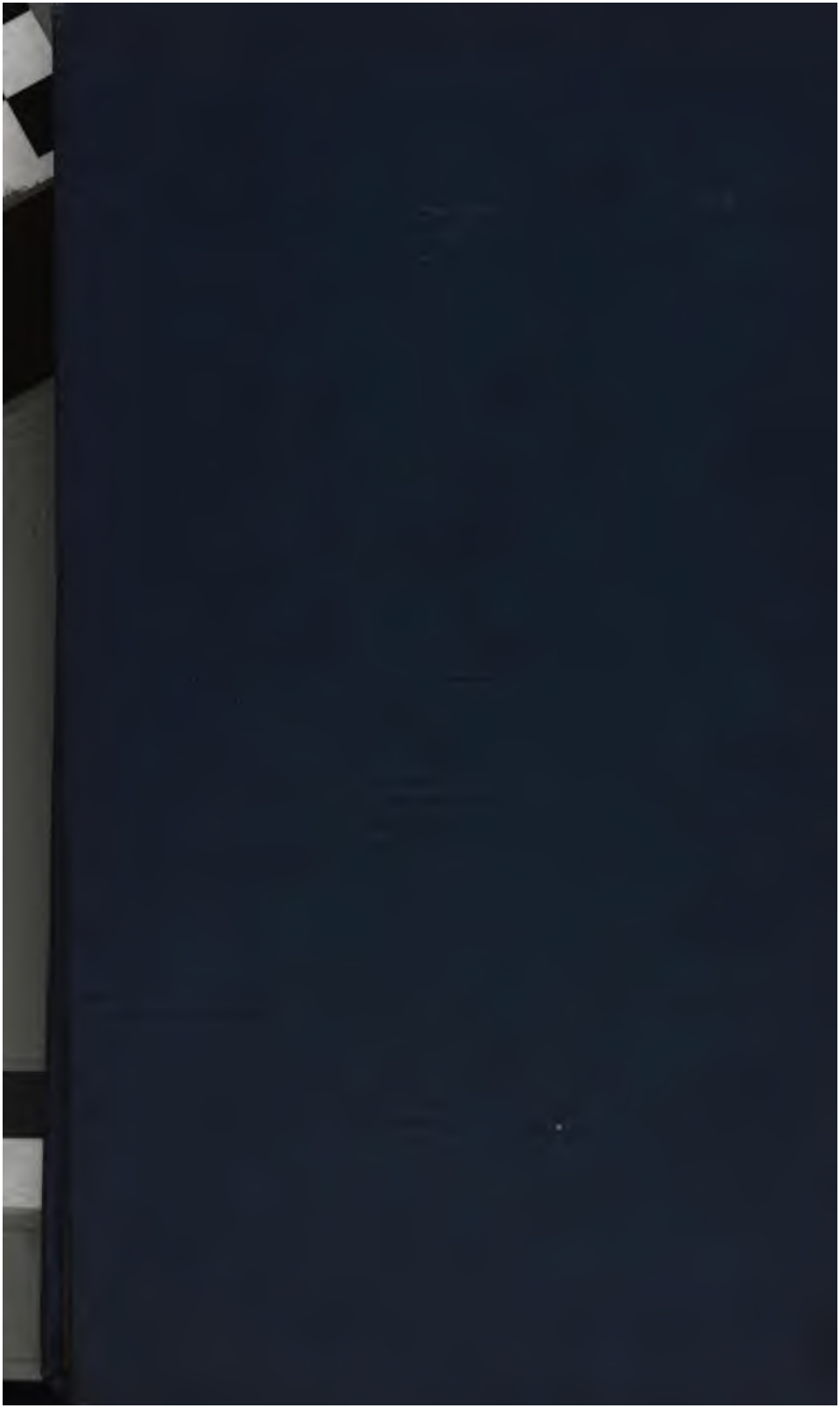
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